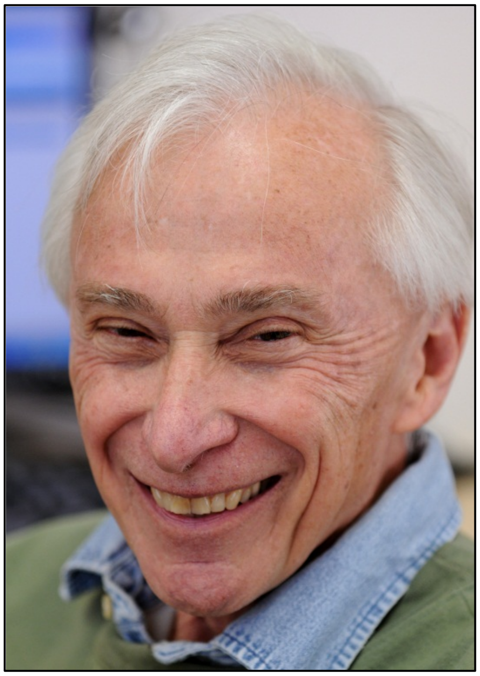

Photo by Robert Paz

THOMAS A. TOMBRELLO
(1936-2014)

INTERVIEWED BY
HEIDI ASPATURIAN

December 26 – 31, 2010

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area

Physics, nuclear physics, astrophysics, national security

Abstract

Interview in nine sessions (December 26–December 31, 2010) with Thomas A. Tombrello, the Robert H. Goddard Professor of Physics, Caltech. Each session is organized around a central topic or theme: (1) early years through college, (2) fifty-year career overview, (3) undergraduate students, (4) Kellogg Radiation Laboratory years, (5) work with Schlumberger research laboratory, (6) Caltech people and personalities, (7) work with national weapons laboratories, (8) ten-year tenure (1998–2008) as chair of Caltech's Division of Physics, Mathematics and Astronomy, and (9) graduate students and miscellaneous topics.

Tombrello opens with his family history, youth, early life, and education, primarily in Texas and Alabama, and his undergraduate (BA 1958) and graduate (PhD 1961) years at Rice Institute. He talks at length about his years in Caltech's Kellogg Radiation Laboratory, including his research into nuclear physics, materials science, and applied physics, and about the science, culture, people, personalities, politics, and economics of Kellogg and the Division of Physics,

Mathematics and Astronomy (PMA) over fifty years. There is extensive discussion of his mentoring work with Caltech undergraduate and graduate students, including his innovative undergraduate course Physics 11 and his perspectives on student life at Caltech. Of particular note is the discussion of his relationship with S. E. Koonin, who went from being Tombrello's undergraduate advisee to his provost. Tombrello provides a wide-ranging, in-depth look at his ten years as division chair of PMA, covering research, recruitment, fundraising, collegial relationships within and beyond the division and with JPL, and the evolution of PMA under his oversight. He talks about his involvement in the design and construction of the Cahill Center for Astrophysics (dedicated in 2009) and the Thirty Meter Telescope (TMT) project. He describes his interactions with five decades of Caltech presidents and provosts, institute trustees, and various donors.

Tombrello recaps his two years as research director at Schlumberger research and his several decades of consulting work on weapons, national security, energy, and climate change issues at Los Alamos and Lawrence Livermore National Laboratories. He talks about his foray into earthquake prediction research, his research collaborations in China, his years as Caltech's technology assessment officer, and the emergence of entrepreneurism at Caltech in the 1990s. Anecdotes and recollections of such notable Caltech figures as R. Bacher, J. Benton, H. Brown, L. DuBridge, R. Feynman, W. A. Fowler, M. Gell-Mann, B. Kamb, A. Lange, C. Lauritsen, T. Lauritsen, R. Leighton, C. Patterson, R. Sharp, and F. Zwicky are also part of this oral history.

Administrative information

Access

The interview is partially restricted. Per agreement between Professor Tombrello and the Caltech Archives dated July 31, 2012, portions of this interview are closed for ten years. Closed portions are clearly marked in the transcript.

Copyright

Preferred citation

Tombrello, Thomas A. Interview by Heidi Aspaturian. Pasadena, California, December 26-31, 2010. Oral History Project, California Institute of Technology Archives. Retrieved [supply date of retrieval] from the World Wide Web: http://resolver.caltech.edu/CaltechOH:OH_Tombrello_T

Contact information

Archives, California Institute of Technology
Mail Code 015A-74
Pasadena, CA 91125
Phone: (626)395-2704 Fax: (626)395-4073
Email: archives@caltech.edu

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH THOMAS A. TOMBRELLO

BY HEIDI ASPATURIAN

PASADENA, CA

NOTE TO READERS

Per agreement between Professor Tombrello and the Caltech Archives dated July 31, 2012, portions of this interview are closed for ten years. Closed portions are clearly marked in the transcript.

PREFACE

These interviews were conducted in the week between Christmas and New Year's Day 2010. Heidi and I sat in my office during this quiet period and talked and drank tea with little prior planning of the direction these interviews might take. By way of explanation of their eclectic contents, it is like history itself in that a chance comment would lead us down unexpected paths. I'm sure if we were to do it again, the trajectory of the interviews might be different, and other topics that are not addressed here would have appeared. Even with thirteen hours of interviews, one does not expect to cover an entire life and career.

In correcting the written text of these interviews, I have resisted the temptation to complete stories using details about what has happened in the year since they took place. For example, Nathan Myhrvold did indeed make a lot of money from the Caltech patent I sold him (Session 9). What you have here is, therefore, a snapshot of my life and career taken at the end of 2010.

Throughout this oral history, I have tried to be candid and to present my point of view completely without varnish. From the perspective of those I mention, things may well have looked different. Virtually all of the people I have known have been decent and honorable people. Where I have been negative, I definitely meant to be—take that as you will. My ideal is Horton, my favorite character in all of fiction. "I meant what I said and I said what I meant." [1]

Finally, I have to acknowledge that none of this would have occurred without the encouragement of my colleague of many years, Jenijoy La Belle. She is a lady of charm, wit, and style. Obviously, my interviewer, Heidi Aspaturian, is quite extraordinary—not only as an interviewer but also as an editor. Pulling together thirteen hours of conversation and putting them in coherent form are things that only a talented and dedicated individual could accomplish.

Thomas A. Tombrello
June 6, 2012

[1] Dr. Seuss, *Horton Hatches the Egg* (New York: Random House, 1968).

TABLE OF CONTENTS

INTERVIEW WITH THOMAS A. TOMBRELLO

33-37

Financial downturn in Kellogg in early mid-eighties; split between Tombrello and Fowler group; tensions with Fowler and then-provost R. Vogt over research and funding directions in Kellogg. Receives NSF funding to inaugurate materials science program within physics; moves research group to Sloan Annex in 1986-87.

37-40

Growing involvement with oil company Schlumberger: consulting work and service on visiting committee; accepts two-year (1987-89) assignment as director of company's troubled Connecticut-based research lab to restructure lab.

40-45

Creates Caltech Physics 11 class for select undergraduates. Organizes and heads physics staffing committee, which redirects physics department's hiring priorities. Recruitment of R. Vogt to direct LIGO; staffing committee successes and effect on department collegiality. Recruitment of A. Lange and the story behind his decision to come to Caltech. C. Peck's selection as PMA division chair; establishing PMA priorities in late 1980s.

Session 3

46-56

Recollections of "extraordinary" students T. Weaver, S. Koonin, K. Jancaitis, J. Polchinski, W. Zajc, and R. Lee. Origins of Physics 11 class for uniquely creative freshmen. Describes sample entrance questions for class; recalls discussion of social inequality question with Nobel laureate M. Scholes. Successful student approaches to Physics 11 questions. Recollections of student D. Amodei. Comments on Caltech's "narrow" social niche and maladaptive "lost boy" syndrome. Physics 11's structure and average size. Anecdotes and philosophy of giving and receiving favors in Caltech context.

56-62

Physics 11 students T. Riley, B. D'Urso, D. Bacon, G. Chadwick. Placement of class students with Caltech faculty. S. T. Loh as undergraduate and her post–Caltech career. Receives Navas and Feynman teaching awards. Philosophy of teaching undergraduates.

62-67

Creative genius of R. P. Feynman, F. Zwicky, and C. Koch. Assesses overall quality of Caltech teaching; compares Caltech and peer universities. Managing academic expectations of Caltech undergraduates. Career paths of former undergrads D. Osheroff, J. Hall, S. T. Loh. Former USC athletic director M. Garrett's perspective on Caltech students. How teaching students is like training sheep dogs.

Session 4

68-72

Collegial environment of Kellogg Radiation Laboratory in the 1960s. C. Lauritsen's history and leadership of Kellogg; lab's medical research experiments; Lauritsen's participation in inner circle running Caltech.

72-83

Kellogg scientists W. Fowler, C. Barnes, R. Kavanagh, W. Whaling. F. Hoyle. Experimental work on tandem accelerator. Socializing and camaraderie among Kellogg personnel. C. Lauritsen's personality, research agenda, and postwar oversight of Kellogg. R. Christy's work in Kellogg. Tombrello's research on scattering theory and use of early computers. Visitors to Kellogg in 1960s; K. Thorne as undergraduate and young faculty member; M. Schmidt's discovery of redshift of quasars; vibrant intellectual activity throughout 1960s. Budgetary considerations prompt Tombrello to identify new funding sources and redirect some lab research.

83-86

Fowler's establishment of "remarkable culture" within Kellogg. Fowler as visionary; strengths and drawbacks of his management style. PMA division as collection of "large fiefdoms;" power centers within division; evolution of astrophysics program in fifties and sixties; collegial tensions in Kellogg.

86-91

Impact of R. Bacher's decision against big-accelerator physics at Caltech; M. Sands, R. Walker, and A. Tollestrup in Kellogg; potential pitfalls of academic recruitment. M. Gell-Mann's objections to some Kellogg hires; role of physics staffing committee in easing tensions and diversifying faculty recruitment. Recruitment for LIGO project. Comments on PMA division chairs R. Bacher, C. Anderson, and R. Leighton. M. Schmidt as division chair during 1979 split of Caltech and Carnegie Institution astronomy programs. R. Vogt's tenure as PMA chair. Provost B. Kamb successor to Vogt as provost.

91-93

Disagreements over funding and research priorities (1981-82) spark Tombrello's resignation as Kellogg PI. Difficulties with Kellogg grant; faculty resistance to budgetary retrenchment; Tombrello's management style; personnel changes in Kellogg; Tombrello succeeded by C. Barnes and S. Koonin; Koonin assumes management of Kellogg and moves lab in new directions. Kellogg research of R. McKeown and B. Filippone.

Session 5

94-97

Tombrello takes two-year leave (1987-89) from Caltech to head Schlumberger-Doll Research Center: Recruitment by company CEO E. Baird; Schlumberger's flawed purchase of Fairchild Semiconductor; budget negotiations with Schlumberger. Restructures lab operations and initiates new projects, including first explorations of shale oil rock in the Middle East and improved seismic profiling in North Sea. Work with assistant B. R. Perlman and CFO E. Burns.

178-183

Chairs investigative Livermore-based "red team" on counterterrorism; committee report's utilization by Department of Homeland Security and President Obama. Approach to running Livermore committees. Current involvement in Livermore work on alternative energy, climate change, and future of lab's weapons program. Need for lab's directorates to think more strategically. Despite START treaty, "still too many nuclear weapons out there."

183-188

Comments on issues related to aging nuclear stockpiles, including viability, reliability, and challenges posed to national and international security. Counterproductive competition among America's national labs. Praises 1992 Nunn-Lugar Cooperative Threat Reduction Act, which helped peacefully divest former USSR of nuclear and chemical weapons. Appraises performance of U.S. secretaries of energy. Comments on "Climategate." Perspective on future directions for JPL in climate science. Confidence in peak-oil predictions and their application to worldwide coal production. Relative safety of nuclear fission reactors; current state of fusion research and hybrid reactor design; private industry versus public in innovative technology design, e.g., E. Musk and Tesla Motors. Sees "many niches in the energy market."

188-192

Comments on international collaborations on nuclear reactor safety. Security issues as they relate to Israel, Pakistan, and other nations. Threats posed by rogue nuclear/chemical weapons and unchecked proliferation. Livermore's move into intelligence gathering and weapons surveillance.

Session 8

193-195

Inadvertent Development designation as chair of "Physics, Mathematics, and Astrology" sets stage for a decade of fund-raising. Appointed PMA chair (1999) and sets out to craft strategic vision for division and fulfill key division priorities. Early budget challenges.

195-201

Negotiations with Caltech trustee W. Burke and Fairchild Foundation yield major commitment for postdoctoral fellowships; additional fund-raising overtures with Burke in subsequent decade. Grows strong new "string [theory] quintet," but concerted effort to recruit string theorist E. Witten ultimately founders. "Two-body problem" in faculty recruitment. Current status of string-theory field. Obtains funding commitment from C. Cahill for new astrophysics building. Additional fund-raising successes, and a few failures.

201-206

Genesis of Thirty Meter Telescope (TMT, previously CELT): Tombrello and S. Koonin initiate project, with seed funding from Moore Foundation. Reflections on devising successful fund-raising frameworks for TMT and CCAT (Cerro Chajnantor Atacama Telescope). Brings on University of California and Canadian government as CELT/TMT partners, undertakes concept study, and secures additional Moore Foundation support. Recruitment of British astronomer R. Ellis onto TMT project. Comments on Caltech astronomers.

206-213

Establishes (with D. Tirrell) interdisciplinary campus nanotechnology center with Moore and Kavli Foundation support: concept and strategy underlying fund-raising proposal; appointment of M. Roukes and A. Scherer as directors. Establishes (with C. Elachi) joint appointments program between PMA and JPL; comments on Caltech's and PMA's relationship with JPL and Caltech. Large-telescope competition with Carnegie Institution: process of site selection in Hawaii for TMT; dealing with state and local politics in Hawaii.

213-220

Caltech's academic divisions compared to different nations; collegiality within PMA. Meets with both success and setbacks in efforts to raise quality of mathematics department through high-powered recruitment. "Always go after the best people": M. Mirzakhani, E. Lindenstrauss, D. Calegari, A. Borodin. Undergraduate physics *Wunderkind* C. Hirata returns to PMA as faculty member. Harvard physicist L. Randall's experiences as visiting professor at Caltech.

220-225

Current participation in USC–National Cancer Institute research project; interest in application of Hubbert's Peak to coal reserves. Recollections of A. Lange as "superb scientist;" fund-raising efforts on Lange's behalf with trustees and with W. M. Keck Foundation. Elaborates on philosophy of dealing with donors. Recent NASA administrators and JPL; status of Mars Science Laboratory mission.

225-231

Voyager mission and mission's presiding "genius" E. Stone. Stone as Voyager project scientist, PMA division chair, director of JPL, and head of CARA oversight board for Keck Observatory. Story behind selection of D. Goldin to head NASA. Stone's advocacy of Total Quality Management at JPL. Loss of JPL Mars Polar Lander and Mars Climate Orbiter. C. Elachi as director of JPL. Decision to float Caltech municipal bond issue to underwrite cost of TMT design study. Recalls budgetary coup at Schlumberger.

232-234

Selection of Pritzker Prize winner T. Mayne as architect for Cahill Center for Astronomy and Astrophysics; Mayne's approach to building's design and his working relationship with Tombrello; reaction of donor Cahill and others to Cahill building; thoughts on how buildings evolve.

Session 9

235-238

Recalls friendship with 1960s Kellogg graduate students L. Senhouse and A. Bacher. Current friendship with Caltech trustee S. Malcolm. Nuclear spectroscopy work with graduate students in 1960s. Begins transition into other research areas in late sixties.

238-241

Collaborates on field experiments in earthquake prediction in 1970s. Project's progress and setbacks and unfortunate loss of funding a year before 1987 Whittier Narrows quake.

241-246
Recalls visits to China and consulting work there, starting in 1979. Growth of Chinese interest in materials analysis research. Visits hydroelectric dam project in Lanzhou. On 1982 trip to Shanghai, daughter Kerstin meets U.K. Prime Minister Margaret Thatcher. Return to China in ’97 to witness transfer of Hong Kong to Chinese sovereignty. Thoughts on China’s likely future political and economic evolution. Comparisons of China and India, New York City and Shanghai.

246-253
Comments on Antarctic AMANDA neutrino-detection project. Recruitment of astronomer F. Harrison onto Caltech faculty and her work as PI on NuSTAR spacecraft. Astronomer A. Sargent’s scientific and administrative career trajectory at Caltech. Former Caltech professor E. Hughes’ work on LIGO committee. Student suicides at Caltech and Cornell, 2009-10. Thoughts on grooming leaders in science and academia. F. Cordova as exemplar of Caltech-trained scientist/administrator. M. Spiropulu’s high-energy physics research at Caltech.

254-262
Tombrello enticed to visit Bohemian Grove in 2006; Grove’s history and activities; Caltech members; Tombrello elected to membership; Grove environment, ambience, personalities, invited speakers, and restrictions on female visitors. Recap of best and worst talks heard at Grove.

262-269
Bernie Madoff and Brooksley Born as representatives of worst and best of American financial system. Caltech physics PhDs on Wall Street and elsewhere in financial world. Transformative impact on Caltech culture of Caltech Office of Technology Transfer and its first director, L. Gilbert; Tombrello’s experiences as institute’s technology assessment officer; how Caltech students came to embrace entrepreneurism; G. Moore and Caltech. Creating networking opportunities for Caltech students; companies founded by Caltech faculty; E. Musk and SpaceX as role model for future Caltech entrepreneurs.

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

Interview with Thomas A. Tombrello **by Heidi Aspaturian**

Pasadena, California

Session 1	December 26 and 31, 2010
Session 2	December 26, 2010
Session 3	December 26, 2010
Session 4	December 27, 2010
Session 5	December 27, 2010
Session 6	December 28, 2010
Session 7	December 28, 2010
Session 8	December 29, 2010
Session 9	December 31, 2010

SESSION 1[2]

December 26, and December 31, 2010

ASPATURIAN: This is the first oral history interview session with Professor Tom Tombrello. As I always do, I'd like to begin by asking you about your background, your family, and your early experiences.

TOMBRELLO: So we'll begin at the beginning. I was born in Austin, Texas, September 20, 1936. We lived there for three years, and I remember absolutely none of it. I think my first conscious memory, as near as I can tell, was probably the first day we were in Memphis, Tennessee, which would have been sometime in 1939. That was before World War II, and it was a different world.

---

[2] This session combines an interview with Thomas A. Tombrello recorded on December 26, 2010, and a follow-up interview recorded on December 31, after it was discovered that due to an electrical outlet failure, some of the December 26 interview was not recorded. Additional material in this session was originally recorded during Interview #4.

My parents had come through the Depression very well, but of course had been affected by it, particularly my mother. They didn't educate women in those days, so she had been a shop girl in Austin. In the first boarding house where she lived, there was a man she truly hated, named Lyndon Baines Johnson. The world was much smaller in those days.

ASPATURIAN: Your mother knew LBJ?

TOMBRELLO: Oh, yes. My grandmother liked him, my mother detested him, and that was a lifelong thing. They knew one another when they were young.

ASPATURIAN: Let me step back for one moment. How deep into Texas do your family roots go?

TOMBRELLO: My mother's family probably goes back to the Republic of Texas. There was a land deal. They were trying to sell property to people around the world who were land poor but rich enough to buy property. In this case, burghers in Germany. My mother comes from the hill country Germans of Texas, who came in and discovered that land deals are not always what they are expected to be. There was certainly land. It was certainly quite pretty. But they had the Comanche. They had centipedes, they had rattlesnakes, things that bite and snapped and were difficult to deal with. But the hill country Germans were pretty difficult to deal with, too. They survived all that and thrived. My mother didn't go to college, even though she lived just outside Austin and could have easily gone to the University of Texas. She had been, I think, valedictorian of her senior class. She had done everything—won prizes for essays, that kind of thing. But girls didn't go to college.

ASPATURIAN: So she was very gifted academically?

TOMBRELLO: She was very smart. My father didn't finish high school. He grew up around Birmingham, Alabama. His father had come to the United States, I think about 1890, at the age of thirteen with a nine-year-old brother, as near as we can tell. They were alone. They were poor. They didn't speak English. They were Sicilians.

ASPATURIAN: Where in Sicily?

TOMBRELLO: Bisacquino. I'm not sure of the pronunciation. It was a little town, associated with mines. He and his brother came in through New Orleans, where they were lynching Sicilians—probably with good reason—and they followed the mines and ended up in northern Alabama, because the iron and coal mines there were still a big thing.

ASPATURIAN: A thirteen-year-old boy and his nine-year-old brother?

TOMBRELLO: Yes. Some small fraction of kids who were thrown out in the world that way survived. I learned later that Lee Iacocca's father was twelve when he came to America. It was not just Sicilians. It was all ethnic groups who came from places where poverty and starvation made them desperate. If they had a boy who was big for his age, or aggressive for his age—probably the case with my grandfather—they threw them out in the world. Their job was to save the family, which they did, surprisingly enough. But, of course, if they hadn't saved the family, I wouldn't be here being interviewed. So it was a self-fulfilling prophecy.

ASPATURIAN: Anthropic principle of the Tombrello family.

TOMBRELLO: Absolutely. Weak anthropic principle. So by means no one quite knows, my grandfather fought in the Spanish-American War. He was a muleskinner, a muleskinner being a person who handles the mules. Things were pulled, hauled, whatever, with mules, in those days, on the battlefields, in the coal mines. He started handling the mules because presumably that is what he had done as an even younger child in Sicily. By means that no one ever talks about, and that probably nobody even knows about anymore, he ended up owning a small mining town. So "we" owned a company store. We paid people in company money. And we were not exactly an equal-opportunity employer, I gather. But we didn't learn that in Sicily, I suspect; we probably learned it from the people we replaced in northern Alabama, because that was the way of the world then. We could go more into this later, but in some ways, it's a bit of a distraction. But it shows that the people I come from were survivors and very tough. As for my mother's side of the family, you might say the only person my grandfather felt was as tough as he was, was my little German-American mother.

My mother met my father in Austin. He had come there by another strange route. He had grown up in Alabama, escaped the family business, and become a stock boy in a variety store—Silver's [Isaac Silver & Brothers] in Birmingham, or in the Birmingham area. He did well: He discovered that you could actually have innovative ideas and people might adopt them. He discovered that the store had display windows, but they didn't do much with them. He got free posters and so forth from travel agencies to give the displays a bit more life; and he changed them often so that people would notice them.

ASPATURIAN: What era are we talking about?

TOMBRELLO: We are talking about somewhere in the twenties. He was born in 1908, so he must have been a teenager when the storeowners or managers discovered that this kid had some talent. They shipped him to New York, where the company headquarters were—again to manage stockrooms. And when a store became available out in Brooklyn, somebody said, "Why don't you try the kid?"

ASPATURIAN: So your father had shown a precocious ability in various areas of—

TOMBRELLO: Retail merchandising. When that Brooklyn store, a small one, opened up, he was sent out there. In those days, stores stayed open a long time. They stayed open at least six days a week, and in this particular section of Brooklyn, for reasons that should be obvious, they were open on Sunday and closed on Saturday. It was a Jewish neighborhood. The company was a nationwide chain, similar to but smaller than Woolworths, and quite adaptable. They knew how to work the environment. My father learned a lot in New York. Remember, he had never finished high school. Sicilian immigrant families didn't prize education that much; they prized business success. So he learned a lot in New York. There were museums; there were things to see. Being there was an education in its own right. But then the Depression came. Silver's had trouble like everybody else, and they were going to have to close some stores across the country. My father had a good job during the Depression, but in some ways an awful job, in that it was to get in his car, drive to a town that had a branch of the store, sell the merchandise in the store, sell the counters, whatever he could, get out of the lease, fire the employees, and drive to the next town.

ASPATURIAN: Oh, my.

TOMBRELLO: This proceeded until he got to Texas, where the East Texas oil field had come in. This was probably in the early 1930s. There were bluebonnets and Indian paintbrush and buttercups on the hills. It was springtime in Texas, and the Depression was somewhere else. My father sent a telegram back to the company. You didn't make long-distance calls in those days, you sent a telegram. He says, "Hi. If you don't mind, I plan not to close the Austin store. I plan to run it." The answer basically came back, "If you say so, Tom. Sounds like the right thing to do." The rest is history. He met my mother. As I mentioned, she had come to Austin to be a shop girl, and she was in a boarding house with LBJ, whom she detested.

ASPATURIAN: He was a fellow boarder?

TOMBRELLO: I think he ate there but did not live there. I'm not sure. He used to court the old ladies in the Austin area and would stop in at the boarding house and have coffee with old Mrs. Marcuse, and she adored him. This was perfect.

My mother did not like her own family very much, because they had denied her an education even though she had been an extremely good student. Very good grades, student government, winning essay prizes, but you didn't educate women then. And so the story—her story—goes, she was walking down the street with a girlfriend, and they saw my father. And the girlfriend says: "That's a very handsome man." And my mother says: "Yes, I'm going to marry him." She got a job as his cashier, and she did indeed marry him. Family story was she got to take the proceeds from the day to the bank in a bag. You collected the cash at the end of the day and took it to the bank and deposited it. I said, "Dad, didn't it ever bother you that you sent this tiny little woman off to the bank with the money?" He said, "Tommy, your mother had a twenty-five-caliber automatic pistol in her purse and she really knew how to use it and everybody knew it." So my mother was something else.

My mother raced whippets before she knew my father. That was one of her hobbies. To keep the whippets sharp, they would let them chase a live rabbit—a Texas jackrabbit, which is a pretty good race even for a whippet. There were pictures that I saw of those days. They were an interesting crowd—the flappers, with the hip flask or the tiny silver flask under a very short skirt. My mother was definitely a flapper, but with a 25-caliber automatic in her purse, and she knew interesting people. My mother rode, and some of their friends were in the polo crowd. They knew Cecil Smith, who was a ten-goal man ["Texan...Cecil Smith {was} perhaps the greatest player in the history of a game more than 2,000 years old." From the 1994 *NYT* obituary.—*ed*.] They used to go out to the places where the polo ponies were kept when they weren't having polo matches. They knew all sorts of people—like LBJ. But when you're a little kid, you never ask the right questions. Why did my parents know people like that? This was not the top of Austin society by any means, but it was a bunch of young people who were having a good time.

That's really the story of my parents in Austin, except for one thing. I said it was springtime and the bluebonnets were on the hillsides, and my father saw this as Heaven after being through the Hell of all these places that were in desperate financial shape. He just felt Texas was the only place he was ever going to be, and he was an adopted Texan from the day he got there. Eventually he took the time to take oil-painting lessons, and he only painted one thing, bluebonnets and Indian paintbrush on hillsides. I just wish I had had the good sense to get one of those paintings away from one of my relatives. At this point in time, I would really love to have one of those paintings. I can't remember how good they were, although it was clear what he painted, and that's all he painted.

Years later, I was talking to Annette Schlumberger [of the Schlumberger oil family]. We had scheduled some sort of event down at her estate in the South of France, and at dinner I was telling her my father's story about Austin, and she says, "Well, Schlumberger got thrown out of the Soviet Union. They had been a big asset for the company. But we were thrown out and had to come back to Texas and try to make things work." And she says, "I remember being a young girl in Austin and riding up those hills through the bluebonnets, and I felt the same way your father did. We were coming back into springtime after a very low period in the company." My bread-and-butter gift to her—which I hope she did something with—was large sacks of bluebonnet and Indian paintbrush seeds. I would like to think that somewhere in the South of France, lupine and Indian paintbrush are on those hillsides

ASPATURIAN: And from there, back to you.

TOMBRELLO: OK. I was born in Austin, Texas, and lived there until I was three. I was an only child. This was the Depression. Lots of us were only children. When I was three, we went to

Memphis, and I believe my first memory was probably of the first day, or the first night, we were in Memphis.

So this is before World War II, probably somewhere in 1939. I was a little kid. They didn't know what to do with me. There were very few children in the neighborhood then, and they put me in a private kindergarten, although I was only three, probably almost four. The school tried to teach me how to read and write, and they made limited progress. I was just a little young, but I picked up enough, probably, that maybe a year, a year and a half later, I taught myself how to read by just reading letters as shapes. Therefore to this day I cannot spell, but fortunately I know I cannot spell. I read English as if it were hieroglyphics. The words are shapes, and you can learn to read very quickly. My stepdaughter taught herself to read the same way when she was about three and never could spell, but she could read very rapidly and she read very early. So these self-taught readers often are defective in how you deal with words—how you pronounce and spell them. I think I lasted about a month in kindergarten—I was a kindergarten dropout.

ASPATURIAN: Speaking of kindergarten, do you recall the attack on Pearl Harbor? You would have been five.

TOMBRELLO: Oh, yes. That's an interesting story. We were in Memphis. My father was running the local variety store for H. L. Green there, although it was still called Silver's then. They hadn't changed all the names. The store was in downtown Memphis on Main Street, I think—rather a good location, close to the Peabody Hotel, where the ducks used to be led across the lobby at some time during the day to swim around in the fountain there. My father had a bunch of very young, interesting assistant managers. I remember the day very well, because it was a pretty day, clearly in early December, and my family and my father's staff had gone for a picnic at Overton Park. Since I was the only child, I had all these interesting young people to myself. I thought they were wonderful, and they pretended to be very interested in me, and we had a great day. Winding down afterward, we all gathered back in our living room. I was playing on the floor. The radio was playing in the background. My parents and the assistant managers were talking, and my mother said: "Stop. I want to hear what's happening on the radio." And she walks over and turns it up, and we heard the announcement of the attack on Pearl Harbor.

My mother was particularly concerned, because my father's youngest brother, Dominic—Uncle D. to me—who was in the peacetime navy, had been at Pearl the last we'd heard. He was a chief petty officer on the cruiser *Pensacola*. So my mother and father were extremely worried. It was only later that we discovered something that was a surprise to everybody, particularly [Japanese Naval Marshal General Isoroku] Yamamoto. Shortly before December 7th, the carriers had gone off on a cruise, accompanied by the cruisers. They weren't there, and all that was left in Pearl were the battleships. This was quite a disappointment to Yamamoto. He kept saying, "But what about the carriers?" The answer was, nobody knew about the carriers. They weren't there. And my uncle wasn't there.

And so that was a day I truly remember. First, it had been a lovely day. It ended, then, on this note of apprehension. My mother said to the young men who were gathered there, "I think this may be the last time we are all together for such a holiday." And it was true. They were all drafted. They were all sent off various places to do all kinds of things: some into active war-torn zones; some, because they had been in the merchandising business, to the Quartermaster Corps, where they made sure things ran smoothly in moving materiel around the world. But it was indeed entirely predictable that these people did disappear very soon into World War II.

So that fall I started school. I didn't like school very much—I was like some long-term inmate of a penitentiary. I tried to do easy time—don't volunteer for anything; take a book to school every day from the public library. Try to read it, and not get caught, and you learn a great deal if you read a book a day for a long time. So that was my view of the public schools. This was incarceration. I had to do it, and I had to make reasonable grades, but I didn't enjoy it much. I enjoyed playing with my friends. We fought the battles of World War II, and we fought the battles for the American West, the cowboys-and-Indians thing across the backyards of our part of Memphis.

For me the war did not bring hardships. I'm sure it was difficult for my parents. There was rationing. Tires wore out. A car trip was an adventure, because I thought that every trip had at least one or two flat tires. It was always true for us. We went down to Pensacola, Florida, once, right at the beginning of the war. It must have been the summer before first grade. But otherwise, we mostly went to visit my father's family in Birmingham, which was a couple hundred miles away. But in those days you didn't go very fast, the roads were pretty terrible, and you always had flats to deal with, because the tires were old and not very well made in any case.

That's what I remember about World War II: ration stamps and car trouble. In school there were the usual tin-can drives, the paper drives, the grease drives. The grease drives were so successful that one of my father's assistant managers who'd been in the army said part of our problem was how to get rid of the grease, because we had too much to use. I don't know if that was a universal problem or not, but it certainly was true for him. They had so much grease donated they could not cope with it. But things like that—drives of various sorts—really were part of getting people to realize they were in the war. And was the U.S. hit by the war like any other country? Not really. People, of course, all had somebody in the service. They all understood the rationing that they had to deal with.

People were working long hours. My mother didn't go to back to work, but many women did, particularly if their husbands were in the military. There really wasn't very much money coming in. I mean, a buck private was typically getting a buck a day. So it was a tight time. But it certainly changed the situation of the Depression. People had work. You couldn't spend the money, so it was clear that after the war there might well be a bit of a boom from people having saved money and put it away. That prediction turned out to be true when the war ended, but could have turned out to be very difficult in other ways, because we did have inflation. We did have the problems of dislocation. Women did not keep working—we saw the *Leave It to Beaver* kind of family, where the wife was a housewife who stayed home. There were new appliances. You were probably moving into a new house in a new subdivision. You could see that happening in a lot of places. But right after the war it was hard to rent anything. It was hard to get a telephone. I can't say it was a hardship for my family—certainly it wasn't a hardship for a little kid. But I'm sure the people in 1945, '46, had their own problems with it.

ASPATURIAN: By this time you would have been just about in secondary school? At what point did you realize that math and science were—? I assume there was a time—

TOMBRELLO: The time occurred earlier. My parents had bought a set of books called *Childcraft*. They had a couple of big books, one on astronomy. That certainly got my attention. I enjoyed reading that. Mostly, I read fiction. Checked it out of the public library and read it. But I got interested in science, and of course kids had chemistry sets. These were basically little sets of chemicals and instructions on how to do experiments. All the kids in the neighborhood did that. We had all sorts of little fads that ran through the neighborhood. Chemistry sets were one. Archery we got through without too many people being hit by pointy arrows, because we were shooting at everything. There were some accidents. Yo-yos of course were a big thing. We went through the yo-yo phase. It wasn't until much, much later that hula hoops and things like that appeared. That was more high school level.

So I got through the fourth grade in Memphis, and my father was tired from the war and wanted to take a leave of absence or even, probably, just quit and do something else. We moved to Birmingham to be closer to his family. He was going to do something different. But after being close to his family a while in Birmingham, he began to realize that there had been a reason he'd wanted to leave his family and get out of the area. I think my mother always knew it. She knew this wasn't going to last forever. It lasted about a year. The schools in Birmingham were definitely third-rate compared to Memphis. Memphis had good schools; classes were large, but the teachers were good. Remember, there were all these women who couldn't be employed as anything else—very bright women got to be teachers.

ASPATURIAN: That's right, or nurses.

TOMBRELLO: My wife, Stephanie, and I feel we were living in a charmed generation. We were taught by extraordinarily talented women who didn't have very many other opportunities. It was very striking.

I'll jump ahead a little bit to high school. Suddenly some of the men came home from the war, and to give the history of one of my teachers, a guy named Bill Levitt told us this with a straight face. He said, "You know, we all had the GI Bill. We were going to be engineers. We weren't smart enough to be engineers. We then switched into a business major. We weren't really good enough to be a business major, and so finally we got a teaching degree; and here I am." I thought, "Yes, indeed. Here you are." Nice man, entertaining, but you know, a fraction of the ability of the some of the women teaching us. Not that there weren't some men who were good at it. And not that there weren't some women who probably were good at it but were a little bit strange. I had some very strange teachers at that time. But for a long time there were very, very good teachers.

But jumping back to age nine or ten and fifth grade in Birmingham, it wasn't a very good school. It was well behind what we had been doing in Memphis. So I just read everything in sight. My father had always read the *Reader's Digest* and had saved them. So I read the *Reader's Digest* from somewhere in the thirties up until 1945. And though you can talk about the quality of the *Reader's Digest*, it was a comprehensive exposure to the history of the world over a roughly ten-year period. If I have an odd assortment of knowledge, part of it comes from the fact that I just unselectively read my way through all of those *Reader's Digest*s and everything else I could find in the library. It was hard to find public libraries in Birmingham. The school library wasn't much, and so I just read everything in the house.

We were very fortunate in finding our Birmingham house. We lived near Birmingham-Southern College up in the hills on what must have been the west side of town, and the house was beautiful. It can only be described as a craftsman house. It was redwood-shingled on the outside. It was paneled on the inside. It was a beautiful house. It was bought for the princely sum of $13,500. It was on an acre of land, on this hill. It must be that the man who built it had seen craftsman houses, and he built three of them, basically, on this hillside. It was an extraordinary house. It had a semi-finished attic, which was roughly the footprint of the house, and that was mine. That was for Erector Sets and all kinds of projects. The house also had a basement. That's when I realized I was pretty good at fixing things, because my father's idea of something easy to do while he was having his year off was to have a franchise for gumball machines. You put a penny in and a colored piece of gum comes out. Actually, it's quite remunerative. He had many machines. But some of them would break, and I would sort of fiddle with them and some of them I got fixed, and I got paid a little bit for doing it. My uncles would give me pieces of old discarded equipment from the mines, and I would take those apart and try to figure out how to fix them.

I adored my uncles. They were very interesting people. They had done very well during the war. My smartest uncle—Uncle Joe—had gotten out of the mining business and started investing in something called mutual funds, which were new to the time, and he seemed to have done very well at that—as well as he had done at the mines. He was the only uncle whose mine didn't get unionized by the United Mine Workers, and nobody quite knows how and nobody ever asked. I'd always jokingly say, "It's hard to find a union organizer if he's under a thousand tons of rock." [Laughter] I don't know that anything like that happened. But for some reason Uncle Joe never got unionized and always made a lot of money. My Uncle Sam got unionized immediately, because he always met union activity head-on with "Nobody is going to unionize my mines." John L. Lewis got him very easily.

ASPATURIAN: John L. Lewis was quite a force of nature.

TOMBRELLO: Absolutely. But my Uncle Sam figured, "You know, the price of coal is dropping. I could make this more efficient. I can use better technology. I can make money at $5 a ton." The answer is, All you can do is lose a lot of money at $5 a ton. And he did.

I had one uncle who had been in the war. Well, he'd been a quartermaster. Well, no. He had been a kind of gofer for a colonel or maybe a general—a fixer. Uncle Frank. He had one eye—another childhood-accident case, like my father. They each had lost an eye in a childhood accident. Uncle Frank was showing off for this teenage girl he had married—my Aunt Isabel—and he enlisted! He spent the war finding cases of Scotch and that kind of thing for his colonel or general. But then he comes back, and the coal thing is over. He hasn't started any new businesses, and so he flails around for a couple of years, trying to raise peanuts, trying to do a variety of things. None of them work. Then he looked around at the rights-of-way under power lines—you know, the land under them, which has clearly been bought or leased—and he realized that a lot of stuff had grown up there during the war and that it hadn't been cleared. He hired a bunch of unemployed teenagers who previously would have worked in the mines. And so with primitive tools—hand tools—they went out and took contracts to clear the rights-of-way. Brilliant, opportunistic business. As he made money, he began to invest in equipment. Eventually, he took on other projects, like roads and dams and that sort of thing. Very successful at business, and it all started from a bunch of kids out there with bush hooks, killing rattlesnakes and chopping down trees and hauling them off. It was an interesting story.

I still see one of my first cousins, who is about the age of my oldest child. She was on the punk-rock circuit—she's an interesting lady, too—and we talk about Uncle Frank. He was truly bigger than life. After the book *The Godfather* came out, my father used to kid Frank about being the godfather. After he read the book, I believe Uncle Frank took on some of its characteristics. He dressed in a more flamboyant style. I think he was no more into illegal things than other people in the construction business, which is a business known for buying influence and figuring out ways to get contracts that maybe one shouldn't look at too closely. But I don't think he was any different than other people in that business. You think of Kellogg Brown & Root or perhaps the Bechtel Corporation and some of the things they did on a bigger scale. To bring in Lyndon Johnson again, in Texas we used to call him "the senator from Brown & Root." I think the construction business exists because you can get state and government contracts. If people in the business play fast and loose with that, I think that's part of trade. We all enjoyed the continuing story of that when we were watching what was happening in Iraq with Kellogg Brown & Root.

Well, by 1947, we had had one year in Birmingham, and my father got an offer to come back to the H. L. Green Company. He could either have taken the store in Hempstead, Long Island, or the store in Fort Worth, and going back to Texas was obviously a high priority. The Fort Worth store was not finished. While it was being built, he was doing his ordering out of the Dallas store, which was thirty miles away. So we lived in Fort Worth, but Dad was commuting to Dallas. Then somebody died in the company and everybody moved up a notch. My father got the Dallas store, which was the biggest store in the company, and we moved to Dallas. At that point, I was in the sixth grade, discovering that the schools in Dallas were not as good as the schools in Fort Worth, where they were really excellent, or I thought so anyway. For once in my life, in Fort Worth, I actually enjoyed school—for three months.

ASPATURIAN: I have a question. It sounds like you spent most of your youth in the South at a time when it was still segregated. Did this affect your life at all? Was there much awareness of it?

TOMBRELLO: Oh, there was awareness. Everything was segregated. Separate drinking fountains, all that sort of thing. I was a typical Southern kid, who because he didn't know anything else believed in segregation, though it was tempered by the fact that my father ran a variety store, and I would say it was heavily, heavily dependent on the African American population. I wasn't a totally stupid kid. I might have been an unreconstructed Southerner, because all my friends were and you didn't dare appear to be a damn Yankee. First time a new kid comes in they asked you in those days, "Are you a Yankee? You don't talk like a Southerner." I can talk like a Southerner. So I took on all the local coloration and truly believed most of it, except I knew one thing. I realized that, in fact, by keeping the African American population down, you were keeping the economy down. If economic times got better for them, my father might actually make more money, because they'd have more money to spend in his store. So I sort of figured that out—that the South was not as vigorous an economy as it was going to be a few years later, because they had this huge group in the population that really had rotten jobs and were kept in rotten jobs by the unions, by everybody.

Now, during that period one would have thought that it would change, because there was a migration of people from the East. This is a bunch of Yankees moving to Fort Worth, but the amazing thing was, they became Southerners very quickly. I think they took on all the attributes of the local population almost immediately. You didn't see any sign that the people coming in from the East—the "damn Yankees"—had views about segregation different from anybody else's. It was not until 1954, when I just finished high school, that *Brown v. Board of Education* came through, and of course we were aghast that this was going to happen. So, you know, it would have been nice to say that I was liberal in a social sense, but I wasn't, really.

My parents were conservative. I do remember that in the 1944 election my parents, as nearly as I could tell, were the only people in Memphis who voted for Thomas Dewey. Everybody else voted for FDR. My father and mother had voted for him in 1932 and never voted for him again. We were Republicans in a foreign land. Of course, we were registered Democrats. There really wasn't a Republican Party in the South, except in the black community. FDR got a lot of their votes, but the structure was that the local Republican Party was largely African American. As for people from other countries, the United States was white and black.

So, from junior high to high school, I was a chubby little kid for a few years. Smart, wore thick glasses. Funny-looking little kid. And it hasn't totally worn off. But I quit being chubby sometime at the end of junior high. Got interested in sports but wasn't very good at it. Got good grades but didn't really care about the academic parts of school. I was trying to pass for being like everybody else. I was interested in sports and model airplanes and later cars and girls, and it was a question of trying to pass. I did not really associate with the other bright kids.

I associated with the jocks. I wasn't a great jock. That was the part of childhood I felt I had missed in other places by moving around so much, and I really enjoyed that part of it. Of course I knew I was going to go to a good college. My mother wanted me to go to Southern Methodist University, which was in town. But I knew I wanted to get out of town. I was not going to Southern Methodist University no matter how much my mother thought I was. And there were no arguments against Rice—it had no tuition, which was a help. It was a very good school, and one of Dad's friends, a local judge, had basically said, "Of course he's going to Rice!" I'd done well in school; why would I go to SMU? Or even the University of Texas, though that would have been my second choice.

ASPATURIAN: So you went on a full scholarship, basically?

TOMBRELLO: Everybody did. It was a free school. You had about a hundred dollars' worth of fees every year.

ASPATURIAN: So that decision was made.

TOMBRELLO: That decision was made because of friends of my father, like the judge. My father was running a variety store, but he was very active in downtown Dallas things. A friend of his had a store across the street, and it was a very different kind of store. It was called Neiman Marcus—and so Dad was friends with Stanley Marcus. You could be accepted even though you were running a five-and-dime store, if you were a local civic leader. My father was. He knew Bill Thornton**,** who ran the Republic National Bank. They did things for Dallas together. He belonged to the Lions Club. He belonged to the Shrine. It was a great disappointment to him that I was never going to be in the Masons and never going to be in the Shrine, and—though I didn't tell him—never going to be in things like the Lions Club. I wasn't a joiner.

ASPATURIAN: So you went to Rice.

TOMBRELLO: I was off to Rice, majoring in physics. It was the typical attitude one finds at Caltech: "Well, what else is there to major in?" They tried, of course, just like Caltech does, to make you aware of other things that are going on, and probably they did a better job of it. We had a lot of required courses. I have tried to convert the course load to what Caltech had, and I think the required level for the average over four years was about sixty units a term. To succeed at Rice—in those days, it was the Rice Institute—you needed to be a bright kid. Rice was very demanding, and very much connected to science and engineering. I think the entering class was something over 400. There were maybe 1,500 undergraduates, maybe 300 graduate students. The big major was chemical engineering, probably because of the petrochemical industry, although physics was growing very rapidly.

ASPATURIAN: And you decided on physics?

TOMBRELLO: What else was there? The physicists had won the war! [Laughter]

ASPATURIAN: I see. I see.

TOMBRELLO: It was clearly hero worship, you know. American physicists produced radar, or at least developed radar after the British really got it started—the high-frequency radar, the cavity magnetron. I always wondered what the inventors of the cavity magnetron would have thought if they had realized that its ultimate future was to warm leftovers in your microwave.

ASPATURIAN: Interesting.

TOMBRELLO: Oh, yes, but the cavity magnetron was the big thing, and that's what broke open radar, and of course the fact that a bunch of scientists had left Europe, like [Enrico] Fermi, [Leo] Szilard, [Edward] Teller—

ASPATURIAN: So these were all your heroes?

TOMBRELLO: Those were my heroes. They had won the war. Of course, I was interested in science anyway. And I was interested in taking things apart and trying to fix them, and doing little experiments of my own. Nothing very grand, but it was something I really enjoyed. Physics, in my mind, was the way to go. You could continue to play with toys. I've said several times that the family's always thought—my wife, my children—that I'm nine years old. And they're probably right. Nine-year-olds get a lot of fun out of life. Though I remember I was out to dinner with some of my ex-students and their wives up in Seattle a few years ago. One of the wives looked at me and said, "You're not nine years old, Tom." I said, "What do you mean, Kate?" She says, "You like girls too much." [Laughter] I said, "You're right."

ASPATURIAN: With that exception.

TOMBRELLO: With that exception.

ASPATURIAN: Nine going on fourteen.

TOMBRELLO: At Rice I met a local high school girl who had actually come down to Houston from Dallas. I hadn't known her in Dallas, but she had known a friend of the family—a girl I knew, who was just a friend. She'd tell me, "You ought to look up Ann Hall," so I did. You know, people got married young then, and we ended up getting married in 1957, at the end of my junior year at Rice, and maybe eighteen months later we ended up with a baby boy, Christopher.

In 1958 I started grad school in nuclear physics, and there was hero worship again. But first I should mention my summer jobs at Rice. I had them mostly in the gadgets side of the oil industry. After my sophomore year, I think, I worked for a company called Varo, building transformers. After my junior year, I worked for a company that had just been bought by Dresser Industries. Then after my senior year I worked at Shell's [Shell Oil Company] research center in Bellaire, Texas, and that's where I—I can't say I met—I saw, observed, [Marion] King Hubbert, observed Ken [Kenneth S.] Deffeyes. Both peak-oil types.

ASPATURIAN: How did their predictions go down at that time?

TOMBRELLO: People didn't want to believe them. It was only later that it proved inescapable that Hubbert had been right about U.S. oil production, but it took decades. You know, the prediction was made in '56 that it would peak in 1971. This was U.S. oil production; he hadn't made a world prediction. And even then, when it reaches a peak, you don't quite know it's a peak until a little later when it comes down. In fact, one of the things I did when I left Caltech for a couple of years in the late 1980s to run the research lab at Schlumberger [Schlumberger-

Doll Research Center] was get some bright young guy to repeat Hubbert's calculation for world oil. And it was a nice report. I wish I could show it to you, but the company seized it, destroyed it, and essentially there was no more talk of it, because we got basically the same results, although probably with much less precision than Ken Deffeyes got a few years ago. We realized that we were looking at a short period in the history of man in which hydrocarbons, particularly oil, were going to be important. It was a nice report, but the company felt this was not something they were going to show their clients. I wish I had a copy [laughter] but they're gone. They were very careful to grab—I'm sure there's some around somewhere—but I don't have one. It's an interesting story.

ASPATURIAN: So you stayed on at Rice for your PhD.

TOMBRELLO: Let's see, I started graduate school there in the fall of 1958, and I got my master's degree about 1960 and my PhD a year later. Rice was a curious place, in that they didn't attract very good graduate students. I stayed because I had a pregnant wife and I wanted to get out of graduate school quickly and I wanted a minimum amount of trouble. It was good enough in nuclear physics. It may have been comparable to Caltech in nuclear physics.

ASPATURIAN: Interesting. They had some good people.

TOMBRELLO: But they didn't have any good students. But they had a Darwinian approach to students. They let lots of people in, and lots of people got master's degrees and disappeared.

ASPATURIAN: Who was your thesis advisor?

TOMBRELLO: His name is Gerry Phillips. Gerald Cleveland Phillips. He'd been a Rice grad, a Rice undergrad, and had been in naval ROTC and had been a lieutenant commander or something, second-in-command on a submarine in the Pacific. Gerry came back, got his PhD at Rice, and after a year or two somewhere else he'd come back. And he was a bit of a wild man but fun to work for. We shared the idea that if you saw something interesting, just work on it. So I published a handful of papers.

ASPATURIAN: Your thesis topic was—

TOMBRELLO: Thesis topic was on a model of the light nuclei with a theory we had that turned out later was very similar to something [Princeton theoretical physicist] John Wheeler had done, called the resonating-group method, but there were also some experiments in it. My master's thesis was entirely experimental. Again, it dealt with stuff in the light nuclei reactions, polarization of the outgoing particles. It was good training, but I hadn't been at it long enough to really learn to be a decent experimenter, though I had the summer jobs, which had certainly helped a great deal.

I even wrote an undergraduate thesis in mathematical physics with a friend of mine, which we probably could have published, but we weren't very sophisticated about things like that. It was a nice little piece of work, but entirely mathematical physics, and it was based on the fact that we had access to the Shell development computer at a time when very few people programmed or had access to computers. My best friend, Tom Kitchens, and I talked Shell into letting us have access after midnight to an IBM 650 they had. The world was much less formal. They let two Rice undergrads have access to what was then the equivalent of a supercomputer. It filled rooms and had air conditioning, lots of punch cards and stuff. It had about the capability of an HP 15, which is a little pocket calculator that you can buy today for, I don't know, $30, and has been around for almost thirty years. It had 2,000 words of drum storage. It took half a second to divide. That's what I had at Rice, and we made great use of it, because very few of the students knew how to program. We just taught ourselves how to program. We had access to it and other people didn't, so we wrote this paper. We were considered a bit of a prize by one of the theorists in the Rice Physics Department.

THOMAS A. TOMBRELLO

SESSION 2

December 26, 2010

ASPATURIAN: When we stopped, you had just received your PhD from Rice.

TOMBRELLO: Yes. I wrote a thesis fairly quickly and published some papers. Then I was trying to decide what to do next, and I applied for an NSF [National Science Foundation] postdoc. I figured I would take my wife and young son off to Europe. The chairman of the Rice Physics Department, Tom [Thomas W.] Bonner, had been a postdoc here, in Kellogg [Radiation Laboratory]. And he said, "That's ridiculous. You're not going to Europe. You're going to Caltech." I thought, Well, for someone who's never been to California, it's probably just as exotic to go there as Europe, so why not?

ASPATURIAN: How did he make that determination?

TOMBRELLO: Caltech had been very good for him, and he figured it would be good for me. He wasn't my advisor. We yelled at one another. He didn't like being yelled at, so being my advisor wouldn't have worked. But we respected one another quite enormously. He felt he could tell me what was in my best interest, and he believed that was it.

So off we go, in the summer of 1961. This upcoming summer, next August [2011], I will have been here fifty years. And we got here and realized this was an extraordinarily interesting place. I fell into the Kellogg Lab and started doing experiments. They had a new accelerator down in the sub-basement of Sloan [Laboratory of Mathematics and Physics]. They had a bunch of grad students who were probably not getting as much attention as they thought they needed. And I was younger than some of them, older than very few. I was twenty-four when we got here. It was a marvelous time. For the first year, all I did was just keep my head down in the lab, and on weekends we would go to all the free and wonderful places in Southern California. You know, Griffith Observatory, the zoo, the beach. Every place. We discovered a bigger, more interesting world. Texas is pretty dull compared to Southern California. We just had a wonderful time. And you fell into a kind of community of people. There were lots of visitors. Young visitors. There was a swimming pool; kids were in swimming classes. We only had one, but Susan was on her way. She was born in the summer of '62.

ASPATURIAN: This would be your second child?

TOMBRELLO: Yes. We loved it here. We were the same age as the students and probably just about as impoverished. I was being paid by the NSF, though Caltech didn't pay very much either. We really enjoyed it. But after about a year, I wondered where it was going. I realized that my advisor had been putting my name out on the street. I began to have people contacting me, like, "What are you going to do next?" With a little bit of looking around, we ended up deciding to go to Yale—

ASPATURIAN: A really different choice.

TOMBRELLO: I was hired as an instructor. We were going to leave at the end of the summer of '62. But then I got an extension, because we were doing some experiments I was really having fun with.

ASPATURIAN: Whom were you working with at this time?

TOMBRELLO: I was working more or less by myself and with some of the grad students. Andy Bacher, who became one of my very first students, was the son of the head of the Division of Physics, Math, and Astronomy [PMA], Robert Bacher [professor of physics, emeritus, d. 2004]. During that period, Bacher got kicked upstairs to provost. Carl Anderson [professor of physics, emeritus, d.1991] was about to become division chair.

So, anyway, we leave for Yale in the middle of the winter, driving a somewhat old car, and have all the adventures you can have with freezing weather, old car, and a little girl who's a few months old with an ear infection screaming in the back seat. It was interesting. I admit that Yale didn't look all that great. We arrived in New Haven, old black snow piled up along the curbs, trying to find a place to live. We rented a furnished house—furnished in early Salvation Army or worse. I was making probably even slightly less money as an instructor than I had at

Caltech, although very quickly they promoted me to assistant professor. I was teaching and enjoyed it. I taught in graduate school. I had an assistantship my first year. That was OK. Then I had an NSF grant—you could get paid extra if you taught, and I taught using the Houston method. I'm going to have to take a slight digression here—William Houston was president of Rice at that time. But before that, he'd been chairman of the physics division at Caltech and had invented a way of teaching, which was that the kids went to the board five hours a week, and you were graded on how well you did at the board.

ASPATURIAN: Meaning what, exactly?

TOMBRELLO: Meaning you did whatever problem you were sort of assigned at random. Basically, you learned that you had better try to work every problem in the book, because you never knew what was going to happen. People like Bob [Robert B.] Leighton [Valentine Professor of Physics, emeritus, d. 1997] had been a product of that approach in an earlier generation at Caltech. You learned to think on your feet. You also learned strategy—say, somebody's at the board and can't work the problem, and you know there's a problem coming along that you might not want to get. *You* jump up and volunteer to finish the problem of the guy at the board. So you learn gamesmanship in addition to learning how to work the problems. I not only took it at Rice; I taught it at Rice, when I was in my last years as a grad student, which was great preparation for my PhD oral. Because those things tended to be shootouts at Rice.

ASPATURIAN: It taught you to be very fast on your feet, I would think.

TOMBRELLO: Sneaky as much as smart, but yes. You worked what you could and tried to present it as though it were the whole problem. So I'm at Yale and I'm teaching, which I did not mind. The students there were not as good as the ones at Caltech, but they were not bad. I mean, Yale was a great place. But I realized I was not happy. So I began to negotiate with Willy [William A.] Fowler [Institute Professor of Physics, emeritus, d. 1995] about coming back as a postdoc.

ASPATURIAN: So you were willing to give up an assistant professorship—

TOMBRELLO: Let me go on about that. Today it seems like a big deal. I was negotiating with Willy, not letting the people at Yale know what was going on. I went off to a conference at Gatlinburg [Tennessee]. In those days, people shared rooms, because we didn't have any money. No one had any money. My roommate comes in and hears me on the phone negotiating with Willy, and he realizes, you know, I'm in play. The next morning, like I was Cinderella after the ball, three assistant-professor offers had been pushed under the door of my room. One of them was from Stanford, and I can't remember where the other two were from, but they were the sort of thing that would get your attention. Of course, I was determined to go back to Caltech as a postdoc. It was not that I was such great stuff, although I think my advisor had sold people on that idea. It was more that the times were changing. Kennedy was in. There was money going into science, a lot of money. Small labs, small accelerator labs, particularly in nuclear physics, were being built. They didn't have anybody to run those labs.

ASPATURIAN: You were in a hot area.

TOMBRELLO: I was in a hot area, at the right time, and with a great agent—my advisor Gerry Phillips, at Rice, who basically said, "You ought to take a look at Tom. He'll get something going there." It's probably true. Anyway, I knew that even if I made a mistake about Caltech, there were still jobs out there. Then, of course, I had to tell Yale before somebody else told them. The chairman of the department basically said, "Nobody leaves an assistant professorship at Yale to go be a postdoc again." I said, "You missed the point. I'm doing it." He said, "Yes, you are. Are you sure you're not making a mistake?" I said, "I don't think so." Yale was sufficiently curious about all of this that for years we were invited back to spend a month or two in the summer there, which, after I met Stephanie—which is farther along in the story—became a good deal, because her parents lived not very far away. So my family and I came back to Caltech—also in the dead of winter—and I've never regretted it.

ASPATURIAN: What year are we in now?

TOMBRELLO: We are now in early 1964. I spent basically the calendar year of 1963 at Yale, at the end of which I was down in Dallas, because my mother died and my father had a heart attack. It turned out to be an interesting transition period of getting out of Yale, getting to Caltech, getting my father's situation stabilized, getting my mother buried. Fortunately there was enough money. Nobody was rich, but nobody was so poor that you had to worry about how you handle all of this.

ASPATURIAN: May I interpose a question? Were you in Dallas in the aftermath of the JFK assassination then?

TOMBRELLO: Oh, that's interesting historically, right. Yes, not long afterward. We were at a scheduled meeting in front of the heavy-ion accelerator at Yale. We come out and discover that Kennedy's been shot. This is November 1963. I remember making the remark—now you're connected back to my mother's feelings about Lyndon Johnson—"They better not look too closely at that, because they might find that the vice president had something to do with it." Probably uncharitable, but Johnson was having his own hard times at that point with things in Texas. I went down to Dallas roughly at Christmastime, leaving wife and two children in New Haven to try to get more or less packed up and get things sorted out after a death in the family. We bring my father out to California for a while. We found a place to live close to the campus. I was almost immediately back in the lab, which is where I wanted to be.

ASPATURIAN: In Kellogg.

TOMBRELLO: In Kellogg. By the next fall, I was teaching Willy Fowler's course. I was a postdoc, and Willy thought, "Hey, he'll teach this course? Let's see what he can do with it." I liked it, but I'm sure the students noticed that I was trying to cut every corner to spend every moment in the lab. I think I did a decent job of teaching the course, but I kept it very, very compartmentalized. The main thing was to spend every waking moment in the lab. It was an eight o'clock class—we had eight o'clock classes then. I would get in early, try to get the experiment set up on the tandem accelerator, maybe leave a student in charge of the last stages of getting the beam cued up, teach the class, and then right after class just run down there immediately. One of the students in the class said, "You do watch the clock.." I said, "I have to. I have to get down there as fast as I can." They were not unsuccessful classes, but for some years I was strictly doing it by the numbers.

ASPATURIAN: When you say that, you mean—

TOMBRELLO: I wasn't trying to win any teaching prizes. I was trying to do a workmanlike job, give the students what they deserved, but remember, I was still trying to get a lot of work done. I became an assistant professor in 1965, but it wasn't even driven by wanting tenure. It was just being driven by the fact that there was great stuff out there. If you didn't take it up, somebody else would.

ASPATURIAN: What were you working on?

TOMBRELLO: Mostly reactions in the light nuclei, some of them of astrophysical importance, because of Willy. Willy was really the man who directed the vision. We'll talk about personalities in later interviews. But certainly the nuclear physics—the spectroscopy—of the light nuclei was drifting into things that were important for stars, how stars made energy.

ASPATURIAN: So you were looking at stellar nucleosynthesis.

TOMBRELLO: Yes, well, not so much nuclear synthesis in the heavier elements. It was mostly the stuff that happened in main sequence stars. These were light element reactions.

ASPATURIAN: Light elements take us from where to where in terms of the periodic table?

TOMBRELLO: Oh, from basically helium and lithium up to maybe neon, somewhere in there. The main sequence stars. The PP [proton-proton] chain, CNO [carbon-nitrogen-oxygen] cycle. It opened up a new energy range, and you had new tools to play with. You had lots of very bright students. That was the thing about Caltech; the students were so good—now we're getting on in the sixties. I'm spending more time teaching, probably. I guess I hadn't quite gotten to teaching freshman and sophomore physics yet. I was moved from the nuclear physics course to teaching classical mechanics, which is a junior-level course, maybe with a few grad students, but they were usually in a different section.

I usually taught one of the undergrad sections. I learned more things about classical physics, particularly electromagnetism, which supplemented the stuff I had been doing in electronics when I'd been an undergraduate and graduate student. So let's see now, we're in the late sixties. We began to take some summers off—in '69 we went up to Seattle to the University of Washington for a couple of months.

ASPATURIAN: By then are you an associate professor?

TOMBRELLO: I'm an associate professor in 1967. But I did not have tenure. They wanted to keep some of us, but they weren't sure they wanted to keep us for a long, long, long time, which is all right. It's perfectly fair. I thought so at the time, too. In fact, I just wasn't worried about tenure. Eventually you start worrying about things like that. So anyway, the summer of '69 we went to Seattle. Summer of 1970, we went back to Yale.

ASPATURIAN: Was this a sabbatical?

TOMBRELLO: They'd invited me, and I basically just took off for the summer to do things I could do at Yale. They paid enough that it was basically a free vacation back East. In the middle of visiting Yale in 1970, I got pulled into a Latin American summer school and left Yale for a couple of weeks to go to South America, which was interesting. The wife and kids went off to Washington to see friends whom we had known when they were students and postdocs, like Roger Noll [former Caltech professor of economics]. By then there are three kids—Karen was born in 1964. I think Ann and the kids probably stayed in downtown Washington, but they saw the Nolls and did the usual things you do when you go to Washington for the first time.

That was an interesting summer. I was still doing nuclear physics. I'm not doing any real consulting yet—the first real consulting was the summer of '71, when I went to Los Alamos. I went there because we had gotten interested in accelerator design in Kellogg. I had some interesting ideas and had some good students who were working on ideas for what you might call heavy-ion accelerators—accelerators where the particles were moving at speeds very far from the velocity of light, not high-energy stuff. It would affect nuclear physics, and it would teach me a few skills that I hadn't had otherwise or force me to learn them. Los Alamos was an eye-opener. It's a lovely place. It's a great place for the kids to go play in the canyons and see bears and go off to Indian reservations. Los Alamos was paying for it all, so it was quite wonderful. We even saved some money from all that. Again, it opened up some interesting possibilities.

We did that again in the summer of 1972. They couldn't find us an apartment. We stayed in a trailer park. I've lived in a trailer park a couple of times.

ASPATURIAN: Interesting experience. It almost sounds like a sitcom premise.

TOMBRELLO: Yeah, right.

ASPATURIAN: Nuclear physicist living in trailer park.

TOMBRELLO: My son and I bicycled all over the Jemez Mountains and even began to start climbing in the Sangre de Cristo range. Now I've got to throw in a little bit about undergrads. In 1971 I had an undergrad named Tom [Thomas A.] Weaver. Before that I'd had Caltech undergrads who were great; but Tom Weaver was phenomenal. Jumping ahead, he won the Lawrence prize [the Ernest Orlando Lawrence Award of the American Physical Society] when he was in his thirties. We published four papers together his senior year. This is wild for an undergraduate. He won the Green prize [George W. Green Memorial Award] at Caltech. Then he went off to grad school at Berkeley. He had been Willy Fowler's advisee. Willy had another advisee, and since having Weaver work with me had gone so well, this other advisee also gets sent down to see me. And his name is Steve [Steven E.] Koonin [former professor of theoretical physics]. This is 1972.

ASPATURIAN: Describe Steve Koonin in those days for me.

TOMBRELLO: He had been in a one-term undergraduate course I taught in nuclear physics. For one of the first assignments—and I think I can probably find it for you later—he turns in something, and I write a note across it that says, "Mr. Koonin, I think this is publishable." [Laughter] I start handing these things back in class, and I look around and say, "Which one of you is Mr. Koonin?" He began to work with me on a theoretical problem that was kind of interesting.

ASPATURIAN: What was he like in those days?

TOMBRELLO: Really bright. Very quick. Enormously quick. Willing to take on hard things. Was he as good as Weaver? I don't know. They were both very good in slightly different ways. Girlfriend appeared—Laurie appeared. They're still married. Laurie was a high school girl, I think, at John Muir High School. Delightful woman. Eventually they began to go to Los Alamos too, so some of the hiking that was being done was with a bunch of grads and undergrads. My son, Chris, and I would climb mountains with them. We were probably in better shape than they were, though. We sort of ground them down. We had a lot of fun climbing up in the Sangre de Cristo Mountains, the Truchas Peaks. Quite a beautiful place. Three 13,000-foot peaks you could climb in a day if you kept whipping yourself along. That was the Los Alamos thing. By the end of that, Tommy [Thomas] Lauritsen [professor of physics, d. 1973]—well, Tommy had had colon cancer in '69, I think, and it was clear that it was recurring and they would not be able to cure it.

ASPATURIAN: Was he overseeing Kellogg at that time?

TOMBRELLO: Willy had been running Kellogg when I first came—though, to be honest, Charlie Charles C.] Lauritsen, Tommy's father, was still alive until 1968, and really everyone deferred to Charlie. Charlie was brilliant. During the war he had been one of the powers in some of the weapons-related stuff, including the solid-fuel rocket project. He had also been one of the primary people in the wartime proximity-fuse project at the Department of Terrestrial Magnetism at the Carnegie [Institution] in Washington. At one point, he moved the whole group, including a very young Tommy and Willy, back there to get the proximity-fuse project started and didn't come back to Caltech until about the time the war really started. Charlie was a mover and shaker: China Lake Weapons Lab, the Aerospace Corporation after the war—there were a number of things Charlie was instrumental in. Very close to [J. Robert] Oppenheimer. Oppenheimer brought Charlie in as one of the cowpunchers. Back to the late sixties. Probably in 1968 Willy got put on the National Science Board.

ASPATURIAN: The National Science Board being a federal agency?

TOMBRELLO: It's the group that oversees the National Science Foundation. It's like a board of directors. By then Kellogg had an NSF grant—we shifted from the Office of Naval Research funding our research to the NSF. As a board member, Willy couldn't be a PI [principal investigator] over an NSF grant. So Tommy became PI of the NSF grant. But then the times suddenly changed. In 1968 we were on this growth curve—all of science was. There was lots of money. And then suddenly there wasn't. The country is trying to fight the Vietnam War, and they're not going to raise taxes. Science began to not grow anymore, and funding even decreased. We probably didn't get as much money from the NSF as we'd gotten from the ONR. It became more of a challenge to run Kellogg.

So Tommy is dying. I'm trying to fit in—this is now probably 1972. My marriage is coming slightly unglued. I have three little kids, a son and two daughters. In the evening I'm going over to sit with Tommy, telling him what's going on in the lab, because I just sort of inherited the day-to-day stuff. There were many more people who were more senior than I was there. So I got tenure in '70 and got to be a full professor in '71, which was more or less on schedule.

ASPATURIAN: You were promoted pretty young.

TOMBRELLO: Yeah, pretty young. There were others who got there faster. I probably got there faster than I deserved, but that's OK. I wasn't complaining. The evenings with Tommy were interesting. He had sort of been given the painkiller of choice, and the painkiller he understood best was gin. So there was a lot of gin being poured. Part of the discussion was drinking with Tommy, and all his old friends dropping by. Some of the old people who had been in the weapons game—and maybe were still in the weapons game—would drop by. Everyone wanted to see Tommy before he died. A slightly prickly saint. A brilliant man who had basically in some ways submerged his own career to keep Kellogg running. Someone once said, "Charlie had one son, but unfortunately it was Willy [Fowler]."

In some sense, Willy was the heir apparent to the vision of Kellogg, and Tommy—Charlie's son—was one of the people who kept it going. But Tommy was a brilliant man—had humor, could figure things out, was good with people. I was always a challenge to him, because I come with my mother's hard edge in dealing with people, particularly people above me in the pecking order, but not so much with people below—but certainly with people above me. We'll get to that again when I get to my interactions with Schlumberger and with the Caltech administration. It's a theme that runs through all this later. Tommy dies in the fall of '73, and Willy makes me PI of the NSF grant.

ASPATURIAN: In Kellogg.

TOMBRELLO: In Kellogg. It was roughly a mid-to-end-year grant, supporting lots of people. Clearly, fewer every year, because we're going through an inflationary period, the stagflation period of President Nixon. The Vietnam War is over, but the effects are still there. So I start bringing in other business that brought money in. I brought in applied things you could do with nuclear physics. It sort of kept the standard of living going in Kellogg. And yet it was bothering some people that this work is pretty applied—not necessarily basic research. By '74, my marriage has come apart. My wife moves out, and I become an unwed mom of three children. My son eventually moved in with his mother. You know, teenage sons and fathers, but I had the two little girls, a nine-year-old and an eleven-year-old.

ASPATURIAN: So the daughters stayed with you and the son went with his mother.

TOMBRELLO: Yes. And the nine-year-old became mistress of the house. I was, of course, head of a major lab at Caltech, and in those days people entertained at home. The Athenaeum was not what it is now; you did not entertain much there in those days. The food was rotten, or at least it was not very good; they didn't have a liquor license. And so we entertained at home. At one end of the long table I would be, and at the other end the nine-year-old. Her sister was socially OK, but she wasn't running the house, and the nine-year-old was. Karen was really something: "Would you like some more wine? It's really nice; I picked it out myself." That sort of thing. We actually had a good time. We didn't have very much money, but we had a wonderful time. We went off to Europe in '75 for three months, rode the trains. I was at the Bohr Institute. Karen became a housewife, and her sister went to an international school. It was good for all of us. We had a wonderful period there. Then we came back. My son had graduated from high school—we're moving right along.

I'm still running Kellogg. Its program has broadened, but I'm the only one doing the broader things. Everybody else is still trying to do what they were doing, but pressures are building because all this other stuff is also going on. These things are not taking money away from the NSF grant, but it certainly means that the extra money they bring in couldn't spread legally into activities that weren't covered by those other grants. We kept the accelerator running; we had more technicians and engineers—remember, in those days you also had a bunch of secretaries. Here I will give the second verse of the Koonin story. Back in the early 1970s, after I had taken over Kellogg, Aage Bohr—the son of Niels Bohr—and Ben Mottelson, who won the Nobel Prize [for physics] in 1975, told me that I needed a house theorist for Kellogg. They were at the Bohr Institute, and they recommended somebody. I said, "He's third-rate." And they said, "Better than you deserve."

ASPATURIAN: What was behind that? That's an odd comment.

TOMBRELLO: Well, basically, we hadn't had a house theorist since [Robert F.] Christy [Institute Professor of Theoretical Physics, emeritus], and Christy had moved into theoretical astrophysics and was not really a part of Kellogg anymore. So we didn't have a house theorist, and we needed one.

ASPATURIAN: But, I mean, to say, "Oh, well, a third-rater is better than you deserve," —

TOMBRELLO: Yes. I said, "Well, I'll grow one," and they laughed at me. I made a prophecy. I said, "I'm going to grow one. And the first thing that's going to happen is you're going to try to hire him from me." And I had this very bright undergrad named Steve Koonin, and I started plotting his future. He was ready for graduate school. I talked to the people at MIT. They had a very good bunch of nuclear theorists. I had no trouble getting him in. His grades were spectacular. He was spectacular.

ASPATURIAN: He got out very quick, too, I believe, from MIT—in three years.

TOMBRELLO: He was a three-year PhD. He worked on probably three things that could have been a PhD. In the summers, he would go to Los Alamos and we would climb mountains together. I wanted to stay in touch, because the plan was to bring him back here. By the summer or fall of '75, he was back here, and the Bohr Institute immediately made him an offer and tried to get him away from me. So I was right. They invited him and I hired him, and I have never regretted it. I don't think it hurt his career. He was a very young assistant professor, and he got hired out of grad school as an assistant professor here, over a number of objections. Oh, that's an interesting story, which I will stick in.

ASPATURIAN: Yes, please do.

TOMBRELLO: In the year 1974-75, Harold Brown [president of Caltech, 1969-77] issued an edict: There will be no new faculty appointments. There were three people on the market that various people in the PMA division thought were the best things in years. One was named David Politzer. One was named Roger Blandford. One was named Steven Koonin. Three of us got together. Murray Gell-Mann [Millikan Professor of Theoretical Physics, emeritus] wanted Politzer; I can't remember who it was that was pushing Blandford; and I wanted Koonin for Kellogg. We swore a great oath that we were going to get those appointments. And Christy was provost, and we were told, "OK, they really do look good. But you're going to have to pay them entirely out of soft money, except it can't appear to be soft money." [Laughter] It was clearly underhanded, but these three kids were just off any scale you could devise. And we figured out a way to do it. In Kellogg, I had had a Sloan Fellowship. Arnie [Arnold J.] Sierk was back here. He'd been one of my students who had come back for an assistant professorship, and he threw in some of his Sloan Fellowship. We raised money every damn way we could to pay Steve's salary, and I'm sure over in high-energy physics they were doing the same thing with Politzer, and over in astrophysics they were doing it with Roger. And we did it! We hired them in a year when there were no other appointments. We hired three hotshots. We were very proud of ourselves, because we had done Caltech a lot of good.

ASPATURIAN: That's a very interesting story.

TOMBRELLO: Yes. We were determined, and history proved us to be absolutely, totally right.

ASPATURIAN: Sort of Caltech's miracle year?

TOMBRELLO: It absolutely was a miracle year. These were people that were just guaranteed—Politzer had already done something very important. Everybody knew it. Roger was clearly doing great stuff, because—to drag Weaver back in there, see, he was my candidate for somebody in theoretical astrophysics, and I realized Roger was probably better than Tom Weaver. Koonin we all know about, because he came here and never got away, even though it was a battle at the time, because outside the division nobody knew who he was. Everybody thought I'd gone way out on a limb. But as soon as he got here, people realized it had been a coup. Everybody tried to hire him away, so keeping him here was tricky. The only thing that probably saved us was that the people who were trying to get him were just not as clever as they should have been. They were working against Sicilian cleverness, or deviousness. [Laughter] I figured out a way to keep him, fight off every one of these offers from outside.

ASPATURIAN: Do you want to detail that?

TOMBRELLO: The main thing was figuring out how to meet offers without entirely meeting the offers. Well, I'll tell you one. Geoff [Geoffrey] Chew was running the Physics Department at Berkeley, and they wanted Koonin. And Geoff made a classic mistake that people make when they negotiate. I am a great believer that you make your best offer first, and it should be better than the person expects. Geoff Chew allowed his offer to sort of ratchet up slowly, and of course the reaction from Koonin was, This guy is trying to get me for the cheapest price he can—which was probably true. But it probably had something to do with the politics at Berkeley in physics, too. By the end of it, Chew was offering more than Caltech was, in terms of tenure and a bunch of things. All Steve got here was an associate professorship without tenure, which is an absolutely meaningless position. It's just like being an assistant professor, but we had made our best offer first. Chew was ratcheting it up slowly, and even though he ended up ahead of us it seemed that he was clearly looking for a bargain, and it made the offer look a lot worse than it actually was. So we kept Steve.

So now we are well into the seventies. By '76, I had met Stephanie, and we got married fairly quickly. I guess we'd known one another six months by the time we were married. We were probably engaged in—she would say six days—but it was probably two weeks. We put the two families together almost immediately. She had a daughter, the one who died a couple of years ago. Kerstin [pronounced Sherstin] just became my daughter, because she was so young.

So we're into the late seventies, and the budget—the financial situation—in Kellogg is not getting necessarily a lot better. The NSF funding was sort of stalled. So the program in nuclear physics, nuclear astrophysics, wasn't growing. There was no way, aside from money for infrastructure, that I could divert money from grants for applied programs into the NSF grant. So pressures are building up, and by the end of '82 it just came apart. Willy was mad. I think it was Nobel fever. He clearly deserved to be considered for a Nobel Prize, but he wasn't getting it, though we nominated him a couple of times [Fowler received the Nobel in 1983—*ed*.]. They got tired of me being PI. It came apart in a very acrimonious way. It could have been done nicely. It wasn't. And so it basically split me off from the group before I split myself off from it. I ended up on the top floor of Kellogg with my own group and bought access to the Kellogg accelerator.

ASPATURIAN: Bought access?

TOMBRELLO: Yeah. Well, you know, you basically pay for time on it.

ASPATURIAN: Like time on a telescope.

TOMBRELLO: Right. The NSF guaranteed a certain amount of time. NSF had given me a certain amount of support, grudgingly. But that's an interesting story in its own right. The NSF people in nuclear physics were acting as if they weren't going to do anything for me. I was a dead man. I was clearly worried about all this.

ASPATURIAN: May I ask the question?

TOMBRELLO: Willy was mad at me.

ASPATURIAN: Over your bringing in outsiders to use the facilities?

TOMBRELLO: Right. He saw the lab moving in a direction he didn't like.

ASPATURIAN: OK.

TOMBRELLO: I wasn't listening. Because I figured we needed the money and had to keep the facility running.

ASPATURIAN: Understood.

TOMBRELLO: So the NSF is trying to keep Willy happy, and the way to keep Willy happy—well, Robbie [Rochus E.] Vogt [R. Stanton Avery Distinguished Service Professor and professor of physics, emeritus] got involved in it in a very heavy-handed way. He was going to drive me out of Caltech. It was very simple.

ASPATURIAN: At this time, Robbie was the provost?

TOMBRELLO: Yes.

ASPATURIAN: So then we're in the early 1980s.

TOMBRELLO: Yes, '82-'83. So we're out at Palm Springs, with the girls, and Stephanie says, "I don't understand why you've got such problems." I said, "You don't? Why not?" She says, "Well, let me just count. The president's science advisor is a guy you consulted for at Los Alamos."

ASPATURIAN: And who was this?

TOMBRELLO: Jay [George] Keyworth. She says, "He obviously likes you a lot." I said, "True." She says, "Your friend, with whom you work and get on really well, Ed [Edward Alan] Knapp, he's head of the National Science Foundation." I said, "True." And she says, "The guy you worked with at Yale, Allan Bromley, is basically running PSAP [President's Science Advisory Panel]"—now it's PCAST [President's Council of Advisors on Science and Technology]. And she says, "I don't understand why you're having any problem. Those people like you, would do anything for you, and they don't really care about Willy at all." [Laughter] I said, "You know, when you put it that way, I should write three letters." So I wrote three letters. And I get this phone call one day from Ed Knapp, who says, "We'll fix it."

ASPATURIAN: Why did you write rather than pick up the phone?

TOMBRELLO: I wanted to explain the whole case carefully, and I wanted to do it in such a way that they all saw exactly the same stuff.

ASPATURIAN: OK.

TOMBRELLO: But clearly they all got together and decided they couldn't fix some things. I was clearly not ever going to be funded in nuclear physics or nuclear astrophysics, but I could be funded in materials science, where some of this stuff had been heading over the past few years. And they basically said, "Hey, it's up to him to make it work."

ASPATURIAN: Meaning you.

TOMBRELLO: They said, "We can make sure he gets money from the NSF." The materials science people were looking for people with fresh ideas and came up with money. That solved it. I had a separate place from Kellogg. I had money to get on the accelerator. It wasn't a lot of money, but it was enough. And so things began to stabilize and actually began to grow. I began to get more funding from the NSF. I guess there was money from the DOE [Department of Energy], money from a whole bunch of things, lots of little grants put together.

It was an accounting nightmare, but, you know, I'm actually pretty good at accounting, and you can make it work. In fact, you can make it work better because it was so confused—Caltech's financial system was garbage, at best. It's not that you could steal money—although maybe people did. But you could move money around in creative ways and get things done that you couldn't otherwise have gotten done.

This goes on for a little while. But then, by late 1986, suddenly some of these grants are not being renewed, and I'd built up the number of grad students. I had a lot of grad students and a lot going on. So a couple of things happened. First thing was, this building we're sitting in now, Sloan Annex, had been a warehouse for the great central shop, which is over where Downs-Lauritsen [Laboratory of Physics] is. I had seized it when I was running Kellogg and gradually lost it as things narrowed in funding. So then Development and Safety moved into this building, and it was a mess. But I think probably sometime in '86 I said to Ed [Edward C.] Stone

[Morrisroe Professor of Physics], who was chair of the division, “We ought to get that. It’s sitting in the middle of your empire. We should own it.” And he got it. He said, “One condition is you have to move into it.”

And that brings in another story. I brought the whole group over from the top floor of Kellogg to look at it. We looked downstairs. The downstairs sort of had offices and doors and stuff. Everybody hated it—it looked like a rabbit warren down there and hopelessly messy. Upstairs it didn’t look like it does now. It had been used for storage. Some of the windows were boarded up. It had a couple of offices. And everybody loved it. The group was very enthusiastic about it. And I was enthusiastic about it. I didn’t quite know why. I went back to Ed Stone and said, “We want the upstairs.” He said, “Really?” I said, “Yes, if you’ll remodel it.” He said, “Oh yeah, we can do that.” I drew up this present design, which was the big open space in the center and lots of little offices around it. The group voted that even with these tiny 100-square-foot offices, they would be willing to be three to an office as long as they didn’t have to give up the open space. I thought, “Yeah, they’re right. But why is it that we have this emotional attachment to the little offices around the central space?”

ASPATURIAN: The communal space.

TOMBRELLO: Yes. In those days the Leakey Foundation had its headquarters on campus, and they used to put out a quarterly report. I picked one up one day, and the first article was on hunter-gatherer societies. It said all these groups are pretty different, except for one thing: They always build their villages the same way. There’s a central open space with a fire pit for communal activities and around it are the huts, where people will retire for privacy. I thought, “Well, we haven’t gotten very far from Africa. We are reacting to exactly the same things. The offices can be small, but people keep their doors open, because they don’t want to miss out on something that might happen in the central space.” This has been an absolutely fabulous design, which we stumbled into. You see a lot of the infrastructure out there [outside Tombrello’s Sloan office]. You see we’ve got coffee out there. We’ve got books out there. We’ve got some files and the refrigerator out there. It really worked.

OK, now we’re about to move into this place. Some of my grants are slipping, and I have too many grad students. In the meantime, I’ve been consulting at Schlumberger.

ASPATURIAN: Schlumberger being—?

TOMBRELLO: An oil service company. It's like Halliburton.

ASPATURIAN: Based where?

TOMBRELLO: Well, let's see, the headquarters in those days were in Manhattan. It was started in France. There were lots of labs there. There was a lab in Japan. Its first real research lab was in Connecticut. I get offered the chance to run the Connecticut research lab. Not entirely out of the blue, because I'd been a consultant for them for six years at that point.

ASPATURIAN: How had your consulting role there come about?

TOMBRELLO: That's an interesting story. The head of the research lab knew Frank Press. I'd known Frank because one of the Lauritsen kids had been married to Frank's son. Frank had just finished his tour as a science advisor to [President Jimmy] Carter and was now back at MIT, but on his way to be head of the NAS [National Academy of Sciences]. So there was this little open space in Frank's career. He was asked to come to Schlumberger to form a small visiting committee. He must have called Willy about joining him. He also talked to John Deutch, who at that point had been in Washington, DC, I believe as an undersecretary of energy and was back at MIT. And Willy didn't want to do it, or couldn't do it, and just told Frank to call me. He asked me if I would be on this committee. Barclay Kamb [Rawn Professor of Geology and Geophysics, emeritus] was also picked. So in early 1981, the four of us go off to Schlumberger. They loved us, except for Barclay. Barclay they couldn't figure out. Barclay is a genius. I love Barclay Kamb; he is one of my heroes. He is truly one of my heroes. But he can be enigmatic.

ASPATURIAN: I think that's a good word for him.

TOMBRELLO: He's a wonderful person. But they couldn't figure him out at Schlumberger. They did not want to deal with it. They could figure me out. They could figure Deutch out; he ended up on their board of directors. Frank always had some connection with them. But I ended up just a routine consultant. More than routine—I was spending forty days a year there. They were clearly willing to pay for me to spend a lot of time trying to advise them on things, and in the middle of that, I get this offer to come run the lab.

Oil had dropped to $10 a barrel, and in the summer of ’86 they fired a third of the research lab. Out in the field, they probably fired many more people. In one day, the research lab lost 30 percent of its people, and the upper brass of the company had done it. They had basically chosen, one by one, the people to keep and the people to get rid of. And, as you can imagine, with a bunch of senior vice presidents doing something like that, it was not well done. It was done very strangely. After that, the lab basically stopped doing anything. The director of the lab didn’t know what the hell was going on. He had been on an upper growth curve of building new stuff, hiring more people, building new facilities, and suddenly one day it’s all over. So he’s rattled. And in the middle of that, they just decide to get rid of him and bring me in.

They brought me in on a two-year contract, because that’s what I agreed to do. I was able to get a two-year leave because by then my friend Barclay Kamb was provost, and Barclay clearly knew what a plum this was. He was not going to stand in my way of trying to get out. He said to me, “I wish it were me [laughter], because the lab’s a gem.” In some ways—you’ll hear more about this later—it was a challenge, because they had fired all but one of the engineers and they fired all the technicians. They had kept a bunch of theorists. And the lab was just nonfunctional.

I realize I am going to have the world’s shortest honeymoon. I’m going to go there, the theorists think I’m going to save them, and I am going to end up firing them. But that won’t happen for maybe twelve hours. By the second day, we are reorganizing the lab, and I’m having heart-to-heart talks with people. I explain to them, “I’ve got to do this. And you’ve got to help me. What you get out of it is, I’ve been at a university for a long time and I can find you jobs. I can find you very good jobs, maybe better jobs than you’ve got now, out in the academic world. You work with me and I’ll take care of you.”

And it worked. I got the lab restructured. I got the budget under control even though it was 30 to 40 percent less than it had been under the previous directors. We started getting stuff done. As I explained to the people, we’re a working farm now, we’re no longer a deer park. But you understand that even with deer parks, someday they’re going to come in and kill the deer.

Now we're going to be a working farm, and they're not going to come in and kill the deer. But we're going to do a lot more stuff. That was the Faustian bargain I made with the people there.

I gather when I was under consideration—I'm jumping ahead—as PMA division chair, somebody must have brought up the fact that I'd spent such a short time with Schlumberger that something bad must have happened—I must have done something terrible. So they sent a letter to the chairman of the corporation, somebody I'd known for years, and he wrote back basically saying, "I don't know how he did it. He fired a whole bunch of them and they love him."

ASPATURIAN: Did you find them all jobs?

TOMBRELLO: Well—oh, no. There were a few who dug their heels in and didn't want my help. They didn't get very good jobs. But the rest of them, oh, yes. There are lots of contacts out there. When we talk about the students, we'll talk about how you get people jobs. Oh, yes. They had reason to love me. They got very nice jobs, and some of them have become senior professors at universities. Nobody likes being fired, but I had to have technicians and engineers, and we had a constrained budget. Anyway, I was there two years and then came back, figuring, Well, this has been sort of the high point of my career, and now I'm back into strained financial circumstances. This is now 1989.

ASPATURIAN: So you're back here in Sloan doing materials and physics.

TOMBRELLO: Yup. And some meteoritic work with lunar samples and the meteorites. But it's all sort of a piece. You're analyzing materials, and some of the materials are from space, some are from reactors, and some are from other places. But before I came back, Barclay, who was provost, and Gerry Neugebauer [Millikan Professor of Physics, emeritus], who was chairing the division, had asked me, "What can we get you to come back? We know we can't equal your salary." I said, "You're certainly not going to be able to equal my salary"—they ended up paying me about half what I was making at Schlumberger. They said, "Is there something you want?" I said, "I'd like to do something that's just about undergraduates and research." They said, "You're already doing this. You've got an incredible record." You know, there was Weaver and there was Koonin and there was [Kenneth G.] Libbrecht [professor of physics, BS '75]. Nobody's stopping you from doing that. We love it!" And I said, "No, no. I want it to be my teaching assignment. I want to design a course that gets the youngest of them into research. I'll take the freshmen, and I will show you that you can get them into meaningful research when they first get here." They said, "Is that all? Hey, that's wonderful—we'll do that." Ah! You'll hear more about that. And so the first class was in 1990. But I was getting the course—the Physics 11 course—organized in '89. And now we're still doing it.

Also, something important happened in '86 that I left out. Ed Stone, who was then still the division chair, came to me and said, "I want to do something different about staffing in the division. It's not clear that we do this now in a systematic way. I want you to take a look at that and run a staffing committee." Because before, we'd been doing it sort of piecemeal, field by field.

ASPATURIAN: When you say staffing, you're talking about the academic personnel?

TOMBRELLO: Hiring professors.

ASPATURIAN: OK.

TOMBRELLO: I took a look at it and thought, "OK, I can do that. But it's going to be different from what they've done in the past. We're not going to have nuclear physics doing its own thing, high-energy physics doing its own thing, and so forth. Too much horse trading; too much emphasis on the best person in a field. I don't want the best of breed, I want the best in show—the best person out there." That should be the motto of the committee—to get the best people regardless of what they are doing. The first thing people tell you when you propose something like this is that you can't choose between apples and oranges. And I said, "We're going to have relatively few appointments. And so I guess you're just going to have to choose, aren't you? And you might even have to choose a kumquat. I mean, you're going to have to break through this, because if you've got one appointment we just can't get into a fight among ourselves. We're going to have to decide which one is the best. We're just going to have to do that."

Of course, one of the first things we did was pick Robbie [Vogt] as head of LIGO [Laser Interferometer Gravitational-wave Observatory], and that whole story is in the Archives oral histories on the LIGO project. But two things happened. First, you begin to hire people outside your major fields. We hired [Valentine Professor and professor of physics] Jeff Kimble—very important. We broke through and hired somebody who was "none of the above," but he was best of show. Hey, you could begin to do that. What happened was interesting. First, some of the old fields began to shrink.

ASPATURIAN: Can you give me some examples here?

TOMBRELLO: Nuclear physics had become very small. High-energy physics is smaller than it was. Today we've grown in some areas. We've grown in condensed matter physics. In those days, we always talked about how we could never get critical mass in these fields. Do we have critical mass now? More than we did then. The other thing was that the division became infinitely more collegial. There was much less horse trading and much more of a feeling of "We'd better get along with our colleagues, because *we're* going to have a hotshot candidate one day, and we want that choice to get through the committee." The division became nicer. I had no idea that was going to happen. I'm extremely pleased by that.

ASPATURIAN: I have a question: Did your mandate here extend to astronomy and mathematics?

TOMBRELLO: No. But it did have an influence on astronomy. Now we go forward to '89. I've come back from Schlumberger. Stone is no longer chair, but Neugebauer is, and he said, "I want you to take the staffing over again." I said, "I've lost two years." He says, "You'll find a winner." Well, I did find a winner. David Goodstein [Gilloon Distinguished Teaching and Service Professor, emeritus, and Caltech vice provost, 1988-2007] had been chosen to be on the selection committee for a new set of fellowships, called the Packards. This was their first year, and all the incredibly bright young people in the United States were being put up for Packards. All those files were in David's office downstairs. I said, "David, I'd like to look at those files." He says, "You know, I'm not supposed to show these files." I said, "Right." He says, "But you know, I'm not here this next weekend." I said, "Well, how about that." So I went into his office and I read all those files. One of the nominees had actually worked for me at Schlumberger.

These are the brightest people in the United States. Some tenured, some not, but they're all young. There's a singularity among them. There's a line in one of the— Did you ever read the Mary Stewart novels about King Arthur?

ASPATURIAN: No.

TOMBRELLO: Oh, they're quite marvelous. There's a part in one where Merlin has put Arthur out in the countryside as a very small child under the protection of Sir Ector and his family. Arthur doesn't know he's the son of a king. The people who have got him know, but their only dealings are with Merlin. Merlin can see a lot of this stuff by looking into the fire, but occasionally he actually goes back and checks on the kid. He goes back and as he comes up to the castle grounds, he sees him playing with Ector's sons, and he says, "I know which one is Arthur. Like a young dragon among lizards." And I say to myself, "I have found a young dragon among the very attractive lizards, and his name is Andrew Lange [Goldberger Professor of Physics, d. 2010]."

ASPATURIAN: What did you see in him, exactly?

TOMBRELLO: I'm not quite sure. But I copy the file—this is the special file. It had an effect almost immediately, because I took it to the staffing committee. I said, "This is the standard." They looked at it and said, "It's certainly some standard. Not many people are going to be up to this standard." I decide I've got to look at him, but he's not answering the phone. He didn't want to talk to me. He's happy at Berkeley. But Tom [B. Thomas] Soifer [professor of physics] says to me, "Have you ever heard of Andrew Lange?" I said, "Yes, I've been chasing him and haven't gotten him." He says, "I can get him down here for a seminar." I said, "And I can pay for it." And so I'm sitting in the back of 201 East Bridge for his talk. Neugebauer comes in: "What are you doing here?" "Staffing committee stuff." "Him?" "Yeah." He said, "Does he know?" "No." He said, "Damned impressive." I said, "I'm going to get him."

I guess a year or two passes. I get a phone call. "I'm Andrew Lange. How are you, Thomas?" I said, "Who are you, Andrew Lange?" He said, "You know perfectly well. You still interested?" I said, "You know, I've been carrying around a dossier on you. It is turning brown on the edge. You know, pieces are falling off. You can at least send me a new CV." Which he does. I said, "What's different for you, Andrew?" He says, "You know damn well what's different for me." He has gone off to a Packard Fellows retreat and met Frances [Frances Arnold, Dickinson Professor of Chemical Engineering, Bioengineering, and Biochemistry] and fallen in love. And he wants to come to Caltech. Or they're both going to go to Princeton.

There's a more complicated story of what happens next, but the short version is, it goes through. We hire him as professor of physics.

That was a clear victory of the staffing approach, but there were others. By then, the staffing committee has become a power in its own right. In some ways, it takes power away from the division chair; it forms another power center within the division. But it also can make the division chair look awfully good.

ASPATURIAN: Sure.

TOMBRELLO: So then [1993], Charlie [Charles W.] Peck [professor of physics, emeritus] becomes division chair. How Peck got chosen is an interesting story. I only know a little about it. Roger Blandford [Tolman Professor of Theoretical Astrophysics 1989-2003] chaired the committee, and he did something he should never do. They gave the administration four choices. Even genies only give you three. He gave them four. They gave them my name—I'm not ordering these; I don't know if there was an order; there probably wasn't—Koonin's name, David Goodstein's name, and Charlie Peck's name. [Thomas E.] Everhart [Caltech president 1987-97] picked Charlie Peck. OK. It's not exactly a ringing mandate. It didn't make me mad; I didn't really care. Made Koonin mad, I think. It was interesting. Not very long after, Koonin became provost, and that particular chemistry was interesting, not that Charlie had anything to do with it. But it didn't make life easier for Charlie, I'm sure, having to deal with Koonin, because they were very different personalities. Anyway, the staffing committee continues to move, and things are happening that were totally unintended, but they are happening. We are building up fields that hadn't been built up. People are becoming more collegial. The place is getting easier to run. Initiatives are being spawned by the staffing committee that then have to be accepted by the division chair.

ASPATURIAN: Such as?

TOMBRELLO: Prioritizing. The astronomers had been after an astrophysics building since 1966. At a faculty meeting after Peck was appointed, I said, "Look. We should have some priorities. Let's make a list. Where is that building on the priority list?" And somebody said, "Not first." "But what's first?" "We need named postdocs, particularly in theory and math." "OK. Hey, Charlie, that's number one. What's number two?" "Need to do something in elementary particle physics." I said, "Got a name?" "Ed Witten." Oh, my! "That's number two." And I'm not the chair, Charlie is. But we're now telling him, "Named postdocs. Endowed." That means a lot of hard work for the chair. Ed Witten—Aha! [Laughter] Yes.

ASPATURIAN: He's at Princeton, I believe.

TOMBRELLO: He's at the Institute for Advanced Study. We will go into this whole story about the wooing of Ed Witten later. Then third on the list was the building—a distant third. The building had been part of the 1980s capital campaign, but we had gotten the Keck Telescopes, so you can't say the division should be crying about that. We may not have gotten the building, but we got the dominant position in astronomy. At that point, they start looking for a new chair, and because of the staffing committee—I think, as much as anything—I got picked.

So that sort of brings my professional history up to about 1998, but I think the stuff that has happened in the last two years, since I stopped being chair, is probably best to tack onto the session we do about being chair [Session 8], because that's all of a piece. OK.

THOMAS A. TOMBRELLO

SESSION 3

December 26, 2010

ASPATURIAN: We are going to talk now at some length about your nearly five decades of experience with Caltech undergrads.

TOMBRELLO: Parts of this have already been included in earlier interviews—that there were perfectly serviceable and talented undergrads before 1971, which covers roughly the first ten years I was here. But then in 1971 Tom Weaver came into my office and sat down. It must have been the beginning of his senior year. I said, "Who are you?" He said, "I'm Tom Weaver and Willy Fowler sent me, and I'm going to stay here until you give me a research project."

I said, "Fat chance. Out!" He says, "Nope. I'm staying. I'm told you are going to give me a research project, and you are going to give me a research project." To make a long story short, we did three different research projects, produced four papers, and I was *totally* spoiled about Caltech. It was an experience that few people have had. The next student like this—I have to get the dates right—was Steve Koonin. He was also an advisee of Willy Fowler. At that point, I'm totally spoiled and I'm thinking, Willy's sent this kid here, he must be bright, let's just see how bright. He and I are in my office, and I'm at the board and I said, "This is what I'm trying to do. And I've always gotten stuck on it. Let me just try to explain it to you and I'll show you where I'm stuck." So I start in on this problem and I realize, at some point, I've gotten past the part where I got stuck and I'm well on my way to getting this problem defined so it can be solved. And Koonin is looking at it, taking notes and making suggestions, and we realize that this is going to be a lot of fun.

At the same time, a freshman comes in named Ken [Kenneth S.] Jancaitis. I had been working on a bit of theoretical work related to an unusual accelerator design. I'd started that in '69 and I hadn't been totally happy with the models of it. Some of them had come from John Pierce [professor of electrical engineering 1970-80], because the model was based on the traveling-wave tube. John Pierce and Bell Labs had designed the first traveling-wave tubes, which are still used, as far as I know, in all communications satellites. It's a high-powered RF [radio frequency] amplifier. But you can also design a slow-wave accelerator based on it for heavy ions. Jancaitis was a freshman, but remember, we're still living off the legacy of *Sputnik*. These kids have had all kinds of advanced material in high school. He comes in and he's taking junior courses in his freshman year. A little bit of complicated classical mechanics with the mathematics to go with it, and he's ready for it.

So now I'm working with Koonin and I'm working with Jancaitis. The Jancaitis story takes on an interesting corollary. It's 1972; I'm sitting at home on a Saturday, and I get a call from somebody at the University of Texas. "I'm stepping down as head of the nuclear lab here. I think you should come here and take my place." There's a named professorship and the opportunity to run a big lab. I said, "Yes, I'm interested. But I've got to talk to this student right now. Can you call me back, maybe in an hour?" So Ken calls and wants to talk about this problem. We start talking about it. Remember, I'm talking to a freshman. I'm talking to a freshman about a hard, publishable problem in accelerator design. And it strikes me during this conversation that if I go to the University of Texas, I'm never going to have a conversation like this with an undergrad. I may never have a conversation like this with a *grad* student—maybe not even with my colleagues. How can I possibly leave this place? So this guy calls back and says, "OK, let's talk." I said, "Too late. There's no way I can go." It's a totally seductive experience at Caltech to meet somebody who is barely out of high school and you're talking with them as if they're a colleague. Caltech's a fantastic place.

So a series of really great students came through. There was one named Joe Polchinski [BS 1975], who is now one of the shining lights in string theory. We published a paper or two together, again on accelerator design. Very interesting kid. Another kid, Bill Zajc [BS 1975], is now the chairman of physics at Columbia—does high-energy nuclear physics. One kid, Roland Lee [BS 1975], and I worked out the business of dating obsidian, using a nuclear technique. I don't think he got a PhD. He's got an MD, and he works in functional MRI and has done very well. I mean, these were just extraordinary kids.

ASPATURIAN: Were these students your advisees, or jointly your advisees?

TOMBRELLO: No. They found me. They found me because I would give them a research project. A real research project that they could publish somewhere.

ASPATURIAN: So not SURF [Summer Undergraduate Research Fellowships] students, for instance—

TOMBRELLO: SURF didn't quite exist at that point. But that's a good story, too. I had these incredibly bright students all at once. At this point, Jancaitis is still working for me, because he was in the same class as Polchinski and Zajc. Weaver and Koonin are gone. Koonin's gone off to MIT, because I'm going to get him back as the house theorist at Kellogg. But I've got too many of these kids to support, and I want to keep them all for the summer. I go to Harold Brown [Caltech president 1969-1977], *my* president. [Lee A.] DuBridge was a great president [1946-69] of Caltech. I knew DuBridge and I used to talk to him. But Harold Brown was kind of *my* president. I can be very candid, and he could be extremely candid with me. Like he could say no. But see, that was the great thing about Harold Brown. You came to him with an idea. He might say no, and you knew it was "No." You'd never have to worry about it again. But if he said yes, he'd say yes right then, and you knew the check was in the mail. So I mentioned to him these students and how I wanted some money for the summer to pay them. In those days you paid them so little. And he said yes. Hey, done! And he says, "But I might have Morrisroe look into getting you some support someplace else. I can't keep doing this."

ASPATURIAN: [David] Morrisroe being the finance guy [vice president for business and finance] at that point.

TOMBRELLO: They got money for me from the Richter Foundation. So I was running a summer thing with Richter Foundation funding, and then, later, when Murph [Marvin L. Goldberger, Caltech president 1978-87] is here, and Fred [Fredrik H.] Shair [professor of chemical engineering, 1976-89] started SURF, I decided that rather than have two of these things competing with each other I would just throw the Richter money into SURF. That's my SURF story. I've never had very many SURF students—I've had a few—but I always had students of my own that were paid for by something or other. Harold Brown basically made a lot of the future happen. And Murph inherited at least one tiny little bit of what supported SURF. They've kept the Richter money, and it's still supporting part of SURF. Morrisroe found it for me, and I always tried to keep him happy. I told him about how good the students were, because, no surprise, they were very good. So, anyway, I had a series of very good students, but it was always something extra I was doing.

And then I went to Schlumberger and came back and Physics 11 got formed. It was a different model. I thought a little bit about it and said, "I'm going to make it a contest. Caltech students love contests. And they love winning contests." That way you run a test of who's got enough initiative to take on a problem that may not have an answer, and who's willing to work on something that's pretty hard when you can't just knock it off like a homework problem. Neugebauer, who was the division chair by then, was very curious. He said, "Well, how are you going to do that?" I said, "Well, they don't know any science. And the last thing I want to do is measure how smart their high school teachers are. I want somehow to make it somewhat independent of preparation but based on willingness to work hard, being creative," and so the idea of the contest problems appeared. I had to come up with problems that are sort of a caricature of science. They usually require being able to get a number out of something. I think it's very important that it's not abstract theory. Can you take a problem that's ill-defined and get something out of it? Don't leave me with a page full of equations. Give me a number. And something where they can just look in any source they want. The only thing they can't do is ask somebody how to solve the problem. They can use a person like a reference book. That's fine, too. Not a closed-book exam—everything's open. Use anything you can get your hands on to solve it. Give them four weeks. I know a lot of them will try to do it the last night. And a lot of the kids who applied don't get it. They've never seen a problem they couldn't work in an hour, an evening, or something.

ASPATURIAN: Let's have a few examples.

TOMBRELLO: I will give you a great example. This is from 1997, so the course had been running for a while at that point. We are in Stockholm. It is early December in Stockholm. You know the story. [Tombrello's brother-in-law, Robert Merton, was co-recipient, with Myron Scholes, of the 1997 Nobel Prize in economics.—*ed* .] Stephanie and I are having breakfast at the Grand Hôtel, and this person we just met, sitting at the table, has heard about the Physics 11 course. He said, "Give me an example of one of the problems." And I said, "OK, I'll give you the example that I used to choose the present class." I said, "You've heard of John Rawls, the American philosopher? And he had a theory of justice, part of which was that in a just society people will accept inequality to the extent they derive some benefit from it." I said, "And the problem is to quantify how much inequality they'll accept." The guy looks at me and says, "There's a name for people like you, Tom." I say, "Well, what?" He says, "Child abuser." I said, "Well, Myron, one of the kids—not the brightest kid but a very bright kid—gets the idea he can use the Black-Scholes option pricing theorem—for which you have just won the Nobel Prize—to solve it where the inequality is the option price." First he sits there, and then he says, "What's the name of that kid?" [Laughter] No more talk of child abuse. [Laughter]

Caltech students can take a lot of abuse. They do interesting things. That's one of the nastier problems, and yet I've asked it now twice, and the second time, a student named Michael Woods—he graduated a couple of years ago [BS 2008]—came up with a very interesting approach. Remember, now it's been long enough that the memory of the first edition of the problem is pretty much gone. He says, "I don't know how much inequality they'll accept. But I know how big an insurance premium a parent will pay to make sure that their kid doesn't suffer inequality." And he says, "It is the amount they are willing to put into the kid's education. And from my personal case, I can give it to you almost to the penny." But the interesting thing about the solution is that it can be put into a form that is independent of culture. In Sweden, you're not paying tuition, but you're paying high taxes, so you can still figure out how much they've been willing to pay. In some societies, it's paying taxes. In some, it's saving for college. In some it's taking loans. "How big a price?" It's quite a brilliant idea. I wish Michael had worked more on it, but it was certainly enough to get him into the course.

ASPATURIAN: How do you come up with these questions?

TOMBRELLO: I read something and I think: I bet that would be something they would find challenging. The ideas come from everywhere. There was one time when I guess I must have been in Boston going down to Schlumberger in Connecticut. I was on highway 84, which you pick up in Hartford and as it goes down to Brewster, and if you're going to Schlumberger you get off a little bit before Brewster. As I sailed through Hartford, I thought, "I'm going to be early." And then suddenly, it's as if traffic congealed. So I think, "Boy, there must be one hell of a wreck somewhere." Traffic had been heavy, but it was moving at the speed limit. And three hours later—something like that, and I was finally at Schlumberger.

And so I gave the Physics 11 students the problem. I give them Hartford and Brewster as being, you know, the two points, and I said, "The traffic is moving at the speed limit. It is at maximum capacity for the freeway. And somewhere in the middle, around Waterbury, they close one lane for about a hundred yards. Nobody breaks the law, nobody exceeds the speed limit. How long does it take me to get from Hartford to Brewster?" Now, there are some kids who go on the Internet and look at the distance between Hartford and Brewster. They find out the speed limit is 65 MPH. They divide the distance by the speed limit, and they get the time, and of course, they don't get in the course. Some people look at it and say, "Hmmm. You've got three lanes and now you've got two lanes. What happens?" Suddenly, for that short period, the available lanes can't carry the traffic. And the traffic's at maximum capacity at 65 mph. What that does is, it sets up a shock wave, but how do you get a number out of it? And some of the kids do really well.

Another problem came from a song that was popular in my youth called "Mairzy Doats." [Singing] "Mairzy doats and dozy doaks and liddle lamzy divy." So, "Mares eat oats, and does eat oaks, and little lambs eat ivy." And I say, it's an ecology problem. You got three species, and you got three plants. What happens? It's a highly nonlinear problem, and you've got to make certain assumptions, and some very bright kids have done exceedingly well on that problem.

So you see, the questions come from everywhere. They're not real science, but they are like science. They don't have simple answers because they depend on what kind of assumptions are you willing to make. It's what you're looking for. You are looking for people, first, who don't give up easily. Some have to have a bit of a sense of humor, because when things don't go well, you have to know how to be willing to start over. You have to be a little creative, but creativity alone doesn't do it. You have to stick to it. It's like science. You really have to keep working at these things. And what you want is this competition between brilliance and willingness to slog it out and get a number out of it. Anyway, there have been a whole series of problems, some more notorious than others.

Now let's talk about Dario. Dario Amodei, a fantastic Physics 11 student from several years back. I could have sold him to any national government as a treasure. I gave him to Steve

[Stephen] Padin [senior research associate in astrophysics], and they worked on designing segmented-mirror telescopes and they published a paper on it—this is a freshman. He won the Green prize for research for that. But by the beginning of his sophomore year, Caltech is driving him crazy. I had to get him out of Caltech. Caltech is a wonderful environment, but if you don't fit the environment, it's a terrible place. So I got Dario a summer internship at Schlumberger in Cambridge, England. He had just finished his sophomore year, and he was now competing head-to-head and winning against the postdocs in seismology. He published two very mathematical, very interesting papers in seismology. And the postdocs are not exactly idiots. One of them had been a Miller Fellow. It's clearly kind of a mistake to send him, because it's hard to sell anybody else to Schlumberger now that they've seen Dario. They know perfectly well that, you know, they all should look like that, right? Well, they don't. He then finished his undergraduate years at Stanford. He's now about to finish his PhD at Princeton, in physics, but doing neuroscience.

ASPATURIAN: So he left Caltech. What was it that didn't work for him here?

TOMBRELLO: If you don't fit into this environment, you're never going to fit. It is a very narrow social niche. Places like Stanford and Berkeley have many social niches. Caltech has one. With Dario, it was very important that he not stick it out. This is a national treasure.

The latest verse on this is that he's now looking for a job. He was being propositioned by Nathan Myhrvold, who runs something called Intellectual Ventures. Nathan was the first chief scientist at Microsoft, and he's got lots of very interesting people who work with him on intellectual ventures. But Dario is considering this, and I say to him, "Look, you've always got to watch out for Peter Pan." He says, "What do you mean?" I say, "Because Never Never Land is very exciting. But some Never Never Lands don't have Wendy to take care of the Lost Boys. You don't want to become a Lost Boy. Some of these places are extremely attractive. But you can very easily become a Lost Boy—in that you are in a place that you can't escape from. You'll have pirates, you'll have the crocodile, you'll have all the wonderful things of Never Never Land, but maybe there's no way out. You've got to watch out for that." And I said, "I'm just going to throw your name out into the world." I put him up for bids. Maybe two weeks go by. I consult for Applied Minds. There's a guy there named Danny Hillis, who invented concurrent computers, was an Imagineer, you know. Danny's wonderful. So I said, "OK, Danny, he's available." I know Elon Musk. I said, "Elon, guess what? I've got another kid, he's the only one in my fifty years here that reminds me of [Richard P.] Feynman [Tolman Professor of Theoretical Physics, d. 1988]." Immediately, Elon's in there. Elon is wonderful. He's totally compulsive. He immediately got Dario's e-mail address and propositioned him. "Come out here. I want to talk to you. I want you to see SpaceX. We're going to colonize Mars. How'd you like to be part of colonizing Mars?" Interesting story.

I also give his name to Larry Page, co-founder of Google. Larry Page waits, maybe an hour, and says, "I'm in." But now he's in with a vengeance, because he has assigned it to somebody I know, named Sebastian Thrun. He's the professor at Stanford who won the [2005] DARPA Grand Challenge—you know, the race across the desert? He also was the inventor of Streetview**.** Sold it to Google and seems to spend a lot of time at Google, and now he's been given the job of capturing Dario. Very interesting. And I said, "Maybe I should have put this kid up on eBay." But that's the other thing I try to do: I try to make the future happen for these kids. I'm not the least bit afraid to put my reputation on the line. And that's what you have to do. That's the reason I could get jobs for those people at Schlumberger, as I described. I'm willing to tell people the absolute truth about this person and say, "I wouldn't be giving you this person if I thought they would fail. In fact, I'm giving you to them, because I think they'll do a lot better than fail. They'll be highly successful wherever they go." I think it's very unfortunate that more people aren't doing that. Most people play it very safe. A few people compete with their students. That's true in some fields, probably more in the humanities than in the sciences. But mentors are always hard to find. You know, there's that marvelous last line in *Goodbye, Mr. Chips*—the Hilton book?

ASPATURIAN: I know the book.

TOMBRELLO: The last line is—he's dying, and somebody in the room says, "It's such a shame that he and his wife never had children." And his last words are, "I've had *thousands*." It's another piece of parenthood. You've got all these protégés. Some of them are your students. I was, I think, unusual at Schlumberger in that I had protégés, and they still call me. They call me from the Far East. They're sitting in some airport and they think, "Let's call the boss and see what he thinks." And it's just great fun. You never know what you're going to find in the e-mail or when you pick up the phone who it's going to be. It's a marvelous experience. It's totally seductive. And, you know, sometimes it's *years* afterward. "You don't remember me?" "Of course I remember you. Doing anything? What are you doing?" Oh, it's quite a remarkable thing.

Back to Physics 11. The structure of the course is pretty well known, but I should get it down. It's a cross between tutorial, which I consider to be one of the most effective forms of education, and seminar. We meet once a week for two hours. Everybody goes to the board and talks about what they're doing.

ASPATURIAN: And the class is how large?

TOMBRELLO: Next year it will be seven. The biggest class we've ever had was nine, which was last year. Smallest class has been four—we've had four several times. It's based on how well they do on those contest problems. The current class helps me choose—the current class in a year has learned enough to know who might survive in this course. In all those years—now, we're talking about well over a hundred kids—there's only been one failure.

ASPATURIAN: When you say a failure—

TOMBRELLO: He knew he didn't fit and he dropped out. And it wasn't for lack of intelligence. It was for lack of determination. Some do a lot better than others. There's one kid who was probably the slowest in his group. He graduated Caltech OK. He went to grad school here and got a PhD in three years with Amnon Yariv [Summerfield Professor of Applied Physics and professor of electrical engineering], which in this day and age is not too shabby. I got a message from him the other day and he was asking me how Physics 11 was going. He got tenure in a very short time at UC San Diego. He probably should be someplace better, but he's got to earn his spurs. He'll take any of these Physics 11 students in the summer.

He's doing well, but you see, there are always advantages to being part of the Caltech community. A couple of years ago, after he got tenure, he called me and I said, "Hey, congratulations." And he says, "I got a problem. I don't have a green card." He's from India. I said, "The university can help you. They've tenured you! Of course they're going to work on your green card." He said, "They don't seem to know what to do." I said, "Well, I know what to do. I'm going to give it to one of our trustees, a woman named Gayle Wilson [wife of former California governor Pete Wilson]." I talked to Gayle and she says, "Oh, I know his congresswoman. I'm in Washington in a couple of weeks, and we'll just fix this." The next thing, he had a green card.

Gayle once asked a favor of me, and I never quite understood why she asked me. She came to me and said, "I'm Swedish. I'm going to be fifty years old. And I would like to be invited to the Nobel ceremonies." And I said, "You're coming to me?" She says, "I think you can do it." I said, "We'll see." But I did. Magic trick! That's a magic trick I can occasionally work.

ASPATURIAN: What did you do?

TOMBRELLO: Oh, I just asked somebody. Somebody I'd done a favor for, several favors for. That's the other thing, you see. You do favors for people without asking for anything in return. I'm willing to do this—and often nothing ever happens. But sometimes, you need a favor or they say, "Is there anything I can do for you?" And the answer is, "Well, since you asked, yes." Like the time I had a young faculty member here from Austria. He had taken a Fulbright early in his career. And one of the conditions is, you have to go back to your country for a couple of years. But Congress can get you an exemption. I had an old friend, and I said, "Karl, can you help me?" He says, "You think Colin Powell's signature will get it for you?" I said, "Yeah!" He says, "I think you'll have it by tomorrow afternoon, the exemption." He runs the Office of Polar Programs—the Arctic and Antarctica programs—for the NSF, which is a big deal, by the way. The U.S. makes no territorial claims in Antarctica.

ASPATURIAN: Right, there's a treaty, I think.

TOMBRELLO: But we have boots on the ground. There's a few-hundred-million-dollar program at the NSF that keeps boots on the ground. Now, they're scientists but they're still boots. So, we're down there. It doesn't look like our army, but it does look like an occupying force. They're doing marvelous stuff. But see, that's the thing. People will do favors for you. You do favors for them. I learned later why Gayle Wilson had asked me. and not David Baltimore [1975

Nobel laureate and Caltech president 1997-2006] about Stockholm. I said, "OK, Gayle, you have to explain." She said, "He thinks I'm a dumb blonde." But she got Pete elected a couple of times. She is such a 150-watt light bulb! She just glows. It helped him. He's the first to admit the role Gayle has played in his career.

I don't think Baltimore took the time to listen. Some people don't tell you how smart they are the first time you meet them. You just have to figure it out in what they've done. Gayle is one of those people. She's very, very smart. She came through with Shayan Mookherjea, and he got his green card and is forever grateful to Caltech.

ASPATURIAN: I bet he is.

TOMBRELLO: Because the University of California, for some reason, couldn't do it. But it was as simple as having a contact with his congresswoman. Gayle says, "Well, fortunately she's Republican."

ASPATURIAN: That was lucky on that particular occasion.

TOMBRELLO: Never kick luck away. So anyway, does Physics 11 miss good kids? Yes, it misses the late bloomers. But I wanted a course where I wasn't trading on what they'd done at Caltech. I wanted people who were unknowns. Would I get the unknowns? And, of course, I get a few unknowns out of high school—kids who walk in. There's one whom I found in the newspaper two years ago. She had her summer job taken away because she'd spoken out against teacher layoffs in the LAUSD [Los Angeles Unified School District]. Aurora Ponce. When I read this, I just wrote to the author of the article that I would give her a summer job. She was starting UC Davis in the fall and definitely needed a summer job to help pay the costs. She had been going to teach high school math in the Jaime Escalante program, and the district just took that job away, because they were mad at her. I hope she had a nice summer here. People who are in power can often do very arbitrary things to people who rock the boat. Aurora rocked the boat. She got here every day that summer on public transportation from South L.A. I would put a bet on her anytime. Those are the kind of people I want.

I had another high school kid who came in. He did not need a summer job, but his aunt had called me. Gail Ellis. Her husband—Jim [James G.] Ellis—runs the business school at

USC. She said, "My nephew's here. Is there something you can do?" "Sure, have him talk to me." Then you sort of realize that he's got these family dynamics and that his family had probably put him in a category that wasn't a good one. He'd been cross-eyed as a kid—I learned that later. His name was Will Galvin. His grandfather was Bob Galvin, who made Motorola what it *used* to be. The family hadn't completely written off Will, but after the summer here we knew him well enough that he got into Caltech and Harvard as an undergrad. He's doing rather well. Finished at Harvard a couple of years ago. Cum laude—not shabby. You get people—unexpected people—and you just have to spot something that other people for some reason had been ignoring. I don't know if they were ignoring Aurora, but they certainly weren't making life easier for her. With Will Galvin, I think his grandfather suddenly discovered he had a hotshot grandson. I found that very interesting. I'd have thought grandfather would have been plenty smart enough to spot he had a great grandson before that. Oh, well. Whatever it takes.

You're sitting in Talulah Riley's chair, when she's here. I got a call a couple of years ago from Elon Musk saying he was engaged to this young British actress—would I take her in? And I did. And she's delightful. She's very smart. I think she's going to apply to Caltech. I hope so. [Professor of literature, emeritus] Jenijoy [La Belle] likes her. I put her in Jenijoy's class, but Talulah has a complicated life. She's in films. She has a husband who needs to use her for Tesla and SpaceX, on occasion. And she's got five stepchildren now. So with students it's always the unexpected. The *Mr. Chips* story. There are thousands, and they don't all look like Talulah, and they don't all look like Feynman, the way Dario does, but each of them has something interesting about them. It's fun watching them grow. There are some where you wonder if they'll grow. But I bet they will. And I think being in Physics 11 probably plays some role in that.

ASPATURIAN: What happens during a typical term in Physics 11? What are the students doing?

TOMBRELLO: OK. They start the beginning of the calendar year—January. They've been at Caltech one quarter at that point. We start just getting acquainted with one another, and early on I start thinking about various faculty they can be placed with for a research project. Caltech doesn't put any money into the summer stipends, which are $6,000 apiece. I've had to scrounge; with nine in the class, that was $54,000. You have to find faculty who will pay it. Remember, it's twice what they pay for SURF. So there clearly has to be some added value for them there.

You have to figure out which ones go where. You have to sort of figure out what the personalities are. How do you put each one of them with the right person, where the personality will not be jarring? They vary in their ability to deal with the undefined. Remember, they're very young, they're all teenaged. So the first term, pretty much, we work on toy problems. I get used to listening to them; then I sort of parcel them out.

ASPATURIAN: What's a toy problem?

TOMBRELLO: Ah. A problem you find in science—something where you can see if they can make any progress in a couple of weeks. Not some big research thing, but "Why don't you look at this?" Sometimes they turn into real problems. Dave [David J.] Stevenson [Goldberger Professor of Planetary Science] has been part of the course for maybe the last ten, twelve years, and he will take a couple of students and be part of the weekly tutorials when he's in town. That's been a big help. Dave's a gifted teacher. He's a brilliant scientist. It's good for the kids to work for him. Some of the kids he's had have done extremely well and published nice papers with him. I could do it without Dave, but it wouldn't be as much fun.

ASPATURIAN: Whom else have you had, in terms of teaching faculty?

TOMBRELLO: None. The students are parceled out to different professors for these research projects I mentioned. Kimble's had one. Zewail's [Ahmed Zewail, Pauling Professor of Chemistry and professor of physics] had a couple. Rob [Robert B.] Phillips [Morris Professor of Biophysics and Biology] had one. How much attention they get all depends on group dynamics. LIGO's had one. But you see one of my former research undergrads was Ken Libbrecht, who sort of runs the student thing for LIGO, and Ken is a true believer. He'll always buy one if I need him. [Axel] Scherer [Neches Professor of Electrical Engineering, Applied Physics, and Physics], of course, has done absolutely better than everybody else at taking a student and getting something just so unexpected and so brilliant. I remember Brian D'Urso [BS 1998] when he was a freshman. He was working on a problem of how you model photonic chips. This had defeated just about everybody except a professor at MIT with a supercomputer. But Brian, I think, figured it out with a Mac, a laptop. I remember Amnon Yariv came in. He was waiting to talk to Scherer, and he met Brian. Brian was a freshman. And it was so funny; I wish I had a video of it. Brian was at the board, and Yariv was taking notes. Amnon is capable of listening to people who know something he wants to know. He was very quiet, and he was taking very good notes. This kid was telling him something. Very interesting.

Brian won the Apker prize [LeRoy Apker Award, APS, 1998]. He's an assistant professor at the University of Pittsburgh now. It's a curious case of a two-career thing. His wife, Vicky, is also a Caltech undergrad [BS 1998]. She was an economics PhD from MIT when he got a physics PhD from Harvard. They have to play the game of where can they both go. I had Vicky's little brother in Physics 11; I think he's now down in the University of Texas. Now, Dario was found by a former Physics 11 student named Dave Bacon [BS physics/literature 1997].

ASPATURIAN: Oh, the "Quantum Pontiff." He has a blog.

TOMBRELLO: Yes. He and I wrote a paper when he was a freshman that he won the Green prize for, on the sliding stones at the Racetrack Playa, which gets a lot of attention periodically. Right now it's getting a lot of attention again. They ought to instrument the damn rocks, now that they can do that. That was less plausible when we were studying it twenty years ago.

ASPATURIAN: Is there an equivalent of Physics 11 in any of the other divisions?

TOMBRELLO: No. It's unique. In many universities they couldn't do it. I remember someone commenting once, "Well, you don't have many students." I said, "Madame, you want Chevrolets? I make Formula 1s." [Laughter.]

ASPATURIAN: Do you ever find yourself saying, though, "Gee, I wish I could teach this course to a group of biologists or planetary scientists?"

TOMBRELLO: Well, I don't try to limit what— Some of the kids end up in those fields. Grayson Chadwick is ending up as a biology major. You've heard of Grayson and the potential solution to HIV, which arose while he was in Physics 11.

ASPATURIAN: Yes, that's right.

TOMBRELLO: But you see, there's where you have to be willing to go out on a limb. He needed some money, and I got him money from the Musk Foundation, because Talulah knew about him. She was sitting in the class with Grayson and knew perfectly well this kid needed money and told Elon, "Hey, talk to him. You might want to give him some money." So as a teenager he got funded. Oh, no. I don't care if they do physics. Remember, one of my undergrads from some years ago was Sandra Tsing Loh [BS 1983]. She was merely Sandra Loh then. This was before Phys 11, but she wrote her senior thesis with me on the technique to make anti-reflective surfaces. She could have been a pretty good physicist. That wasn't her game plan. I wish she wouldn't do that science show.

ASPATURIAN: You don't like *The Loh-Down on Science*"?

TOMBRELLO: It's a waste of her. Now, I think you could organize a very good show with somebody like her. She's very clever. I love some of the stuff she does in book reviews for *The Atlantic*. I love the one-woman shows. I often get invited with the family to some of the openings of the show, like *Mother on Fire*, which I thought was delightful because it was so perfect. It was what happened when [Sandra's oldest daughter] Madeline got to be almost five and Sandra was trying to figure out what to do next. And it was resonant with our society. You go to the shows and the people in the audience have been dealing with it. [Speaks in a nasal voice, imitating Loh's monologues]: "Well, we've got private school; we've got magnet school; we've got parochial school; we've got the public school. What do we do?" And then she has this blackboard where she puts up what looks like a spreadsheet and rates these things.

She can be totally manic in these presentations, but the audience, you see, is in the palm of her hand, because they've *been* there. They identify with the problem, and they find it funny as hell but in some ways so real. You're laughing because you've tried to fight that particular battle. Her first one was *Aliens in America*, which is about her father. I'm always somehow seated next to her father at these events. I guess because we're both weird. I like him. He is weird. I guess he thinks I'm weird too. It took me years to get Sandra the Distinguished Alumni Award [2001] here. I met her sister at the ceremony. She says, "How would you characterize my sister." I said, "It's as if Molly Ivins grew up in the Valley." And she said, "Oh, that's perfect." [Laughter] Another one of my favorites.

ASPATURIAN: Without the Southern accent.

TOMBRELLO: Yes. Remember, though, Molly Ivins could put it on or take it off when she wanted to. She was a Smithie [graduate of Smith College], you know. I remember once saying, "I would love to see a politician have Molly Ivins as their press representative," and then Ann Richards, when she was elected governor of Texas, had Molly Ivins as her press person. Ann Richards—I loved that woman.

ASPATURIAN: Did you know her, as a native Texan?

TOMBRELLO: My father did. They lived in the same neighborhood in Austin. He did not approve of Ann Richards at all, but that's too bad. Actually, they would have liked each other if they'd ever had time to get acquainted. Different politics completely. But anyway, OK, I don't know if we're finished with the undergrads or not. Do you have any questions about the undergrads?

ASPATURIAN: One of the first press releases I ever wrote here was about your winning the John Navas teaching award [1984]. That came back to me as I was going through some background for these interviews.

TOMBRELLO: Yes. It was only given three times. Fred Shair won it, I won it, and Sunney Chan [Hoag Professor of Biophysical Chemistry, emeritus] won it. I am honored to be in such company.

ASPATURIAN: And you won the first Feynman teaching prize [Richard P. Feynman Prize for Excellence in Teaching; 1994] as well. What is your philosophy of teaching?

TOMBRELLO: Try to figure out what the student needs and try to figure out what each student is capable of doing, and don't get locked into either one of those things, because it's an adaptive thing. You know, when I taught big classes, like Physics 1, Physics 2, I realized—you've got to meet their eyes. These are big classes, but you've got to keep looking at them to see if they're following you. You've got to somehow get them engaged in it, and it's hard with the TV generation. They're not used to the TV talking back to them and trying to engage them, particularly in large classes. You've got to somehow figure out if the message is getting through, and if it's not getting through to all of them, can you, on the spot, change the message a little bit and try something different? It's an adaptive process. It's got to be. It's got to be that the professor really is looking for that handle by which you turn the kid. There isn't any one answer, because people learn things very different ways. I remember Richard Feynman saying, "I could never figure out what all these other guys were doing, therefore I did it my own way." And, you know—to paraphrase Frost—that made all the difference with Dick.

ASPATURIAN: "And that has made all the difference." Yes, I know the poem [*The Road Not Taken*].

TOMBRELLO: Dick made a conscious effort to look at problems from a different vantage point. It was deliberate.

ASPATURIAN: That's a hard thing to do.

TOMBRELLO: It's almost impossible. And it takes somebody as smart as Feynman. I'm thinking how to measure how smart Feynman was, because it wasn't any standard sort of smart. It was this way of viewing the world obliquely, and he tried to get there deliberately. I think he worked really hard at that and succeeded in marvelous ways. The other person who was equally creative and from a different vantage point, was Fritz Zwicky [Caltech astronomer, d. 1974]. You've been here long enough that you've at least heard Zwicky's story. Zwicky was unique.

ASPATURIAN: You knew him personally?

TOMBRELLO: Yes. I was never close friends with either one of them. I probably knew Feynman better than Zwicky, but I knew them both. Now the question is, do we have people who are as outrageous and as interesting as those people? Of course we have one that is notorious, and that's Christof Koch [Troendle Professor of Cognitive and Behavioral Biology and professor of computation and neural systems]. Christof is a genius. He looks at things differently. He's taken on a problem that's infinitely harder—

ASPATURIAN: Consciousness.

TOMBRELLO: —than any of theirs, but if you talk about the characters, the interesting personalities, who are just on a different page, Christof is clearly one of them. Eventually I will think of some others to put in there. But the first one that comes to mind is Christof.

ASPATURIAN: What do you think of the quality of teaching at Caltech overall?

TOMBRELLO: Pretty good. Could be a lot better, considering what we have. I think there is too much acceptance of a paradigm that is probably much more justified at a large state university: Large classes, recitation sections. If any place can have a lot of human contact with the students, Caltech would be it. We make up for it really pretty well with things like the SURF program. They get a lot more attention there. And it doesn't necessarily have to be interaction with the professor, though that helps. It's nice to have the professor there to serve as a mentor and as a helper at the next stage when these students need jobs and career advice later on, but working with postdocs or with an eager grad student while doing a SURF is not bad at all. You get much more of their time, and they can often be a big help. At some point the professor has got to write the letters. I don't think people nominate students enough for prizes. Physics 11 students do win a lot of prizes and a lot of scholarships, but I write a lot of letters on their behalf. The willingness to write a recommendation letter or to nominate somebody for something is so important. I think a lot of professors just don't do that. I don't think they're consciously saying, "I'm going to deny these students this extra chance." I just don't think they think about it. And that's a bit sad. I think there should be a lot more competition for these prizes where people are nominated by their advisors. You just have to *do* it. You have to speak up. As I say, you have to put your reputation a little bit on the line. If you believe in the student, speak up for them. Get them a good job. Get them a prize. The prizes mean a great deal. It means a lot to their parents.

ASPATURIAN: That's very true.

TOMBRELLO: I like to reward the parents. I have four children, and I'm pleased when they get some attention. I think of the four, the one who probably got the most attention in college was my daughter Susan, who went to Wesleyan, in Connecticut. She was an English major there. I think she got more attention from the faculty than the other kids did. More even than Kerstin, who was at Harvard—though I think Harvard was a pretty good place. A lot of people ask me where their kids should go to school, and I say, "You know, it depends on how well they know what they want to do. If they really want to be a Marine in science or technology, and if they fit the social environment, Caltech's a wonderful place. But, you know, places like Harvard and Princeton and Berkeley and Stanford are great schools. They have lots of social environments. They have lots of stuff. Kerstin ended up a women's studies major, and an honors major, at Harvard. But you wouldn't have that at most places. I think if they're not sure what they want to do, they should try to pick a place that's just got lots of choices. I'm not against state universities. I think Berkeley, even with all that's happened to it in the last couple of years, is still a great place.

ASPATURIAN: Many state universities are.

TOMBRELLO: Yes. Michigan, for example. I think the University of Texas could easily be in that category eventually. In some areas, it always has been.

ASPATURIAN: You know, every kid, every undergraduate, who comes to Caltech is brilliant. At least on paper and probably in cognitive ability, too. Invariably though, 50 percent of them are going to end up at the bottom half of the class. I think for these young people it must always be something of a shock to discover that they are not forever going to be number one. How have you, as a professor and a mentor, dealt with this over the years?

TOMBRELLO: I've tried to tell them the following: First, life is not in one dimension. Grades are just one dimension. When you go to the annual ceremony where they present the Distinguished Alumni Awards, you notice almost without exception that at most, maybe one of the five or six being honored were at the top of the class. So many of them say, "You know, I was barely an average student here." A few years ago, when I was running the staffing committee for Ed Stone, we looked at somebody in condensed matter physics named Douglas Osheroff, who'd been an undergrad here. He'd written a *marvelous* PhD thesis at Cornell and he was at Bell Labs. He's at Stanford now, but he came for a visit when we were trying to recruit him. Ed

Stone reported this conversation to me that he'd had with Doug. Doug said, "You know, Ed, I feel very uneasy about this. I wasn't really one of the great students at Caltech when I was here." And Ed, without missing a beat, says "Look, Doug. We're hiring you as a full professor of physics. We're not hiring you to be an undergrad again." And a few years later [1996], Doug wins the Nobel Prize and of course deserved it.

ASPATURIAN: I interviewed him for a long piece in *Caltech News* after he won the Nobel, and he told me that story. It just came back to me. He said, "Stone said, 'That's all right, Osheroff. We're not hiring you to be an undergraduate.'"

TOMBRELLO: Exactly! That's the point. These kids grow in very different ways. There was Jim [James E.] Hall, who invented the spoiler on the back of racing cars. At the Distinguished Alumni Awards ceremony [2001], he tried to explain he was barely an average student here. Sandra [Loh]—she was in a different dimension. I think a lot of these kids got something out of Caltech, and the hope is that you can tell them while they're here: "Look around. The future may be in a different dimension than the ones you are being sold. OK, you're not going to do high-energy physics. Maybe you don't *want* to do high-energy physics. Maybe you want to do something *different*. You want to do something that's you." If we can give them that, it's not a question of what your GPA is. Some of the kids with great GPAs are not as successful as some of the ones that got through here somehow.

In the years when I was going around talking to alumni groups for Development—I don't know if they still do that or not; they certainly wouldn't ask me anymore; Development doesn't like me much anymore, nor should they. I would give a little talk about Caltech, and then someone would come up to me at the cocktail party afterward and say, "You know what the most important thing that happened to me at Caltech was?" I'd say, "Mm, you lettered in varsity football and you hadn't even been able to go out in high school?" They said, "Close. It was being in Mr. Ohshima's karate class. He taught me to be a man." I'm not being sexist; the point was, he taught them discipline. He taught them: Here's something hard and arbitrary; you're not going to be able to do it automatically; and if you want to do it, you're going to have to work at it. Do I think that sports play as big a role as anything else here? They certainly can.

I had an interesting conversation a couple of weeks ago. I have a friend; he got fired from his job last summer as athletic director at USC. His name is Mike Garrett. Mike has decided he would like to be athletic director at Caltech. Now, he's a very *unusual* candidate for athletic director for Caltech. They're considering him. I hope it doesn't hurt his chances that he's a friend of mine. Mike is a Heisman Trophy winner. He has a Super Bowl ring from playing with the Kansas City Chiefs. He has a law degree, which he got while he was playing pro football. He has for seventeen years been running an $80-million-a-year program at USC, where every year he had to raise it all from scratch. And he clawed his way up out of the lower part of society and brought his family with him. Could he give Caltech something? Could he give our students something? I think he could, if they're smart enough to hire him. He called me and said, "They put me off until January." I said, "That's because we're academics and we take the Christmas holiday. Exams are over, Mike. They just gave up on the committee work on the athletic director job." I said, "How'd your interviews go?" He said, "Very interesting. Would you like to guess what the student athletes wanted?" I said, "I don't know." He said, "They want to win." I said, "Everybody wants to win. Are they willing to pay the price?" He said, "These kids were. I can help them." And that's the whole trick of teaching.

Well, I have to tell a story. Garrulous old professors are full of stories. When Stephanie and I got married, I was about to go off and do a month of public lectures in Australia, and we took two of the girls—Karen and Kerstin. We were going to different cities in Australia. I was giving public lectures, university lectures, losing my voice. And one weekend they hadn't scheduled us and so we went to a sheep station that was owned by somebody. Have I told you this story?

ASPATURIAN: No. I'm laughing because the idea of a sheep station sounds pretty good to me.

TOMBRELLO: I am a big-city boy. I would have rather been in Sydney, I think, or Melbourne, or Adelaide, but we're out in the boonies with a gazillion sheep. So they ask Stephanie what she'd like to see, and she says, "Well, I'd like to see a sheepdog work, and our youngest"—Kerstin was six at the time—"would like to pat a lamb." The guy says, "Done." And so we go out and there's this little dog of no discernible breed—these are not shelties—a little black dog. It moves a million sheep, and at the end of it there's a lamb sitting in front of our daughter. Stephanie says, "That is so impressive. How do you teach them to do it?" The guy says, "Well, to be honest, they either can do it or they can't do it. But I can sure make 'em better at it." I thought, "I have got my education philosophy." How do you make them better at it? You can't put something there that's not there. But if it's there, you can sure as hell improve it. If you want educational philosophy, that's it. I'm training sheepdogs.

ASPATURIAN: On that note—

TOMBRELLO: OK.

THOMAS A. TOMBRELLO

SESSION 4

December 27, 2010

ASPATURIAN: In this session, we're going to start by talking about your early days here at Caltech as a young researcher in the Kellogg Laboratory. [See also Session 2]

TOMBRELLO: Let's just start with the arrival in Pasadena in August 1961. Wife, small child. Chris was probably about two and a half. We found an apartment up on Washington Boulevard, not too far from Caltech, furnished, at about $100 or $105 a month, which worked well with the NSF postdoc I had. I got into Kellogg; I wasn't sure what I'd do when I got here. I talked to Bob Christy but then decided that maybe the thing to do was go back and do experimental physics. Willy Fowler was very interesting, but Willy was just within weeks of leaving for England for a year. He really liked spending time in England with Fred Hoyle—later, Sir Fred Hoyle. The personalities in Kellogg are very interesting, and the sociology, or anthropology, of Kellogg was interesting. First thing, you discovered that the professors were more approachable than they had been at Rice. My advisor had been quite approachable, but there was a definite barrier between the grad students, the postdocs, and the faculty. In Kellogg it was different. There were lots of young students who were about my age or a little bit older. I was working with them. There was a new tandem accelerator in the basement of Sloan. Sloan had been renovated. It had been the old High-Voltage Lab, and they had turned it into a math building with experimental facilities on the basement and sub-basement level. Low-temperature physics was on the basement level, where John Pellam was running a program.

ASPATURIAN: And who was John Pellam?

TOMBRELLO: John Pellam [professor of physics 1954-1964] was a low-temperature physicist mainly working, I believe, in liquid helium. Some very interesting experiments had been done in that group. It was a small group. Pellam left somewhere in that period, or died, I cannot remember for sure. [Pellam went to UC Irvine in 1965 and died in 1977—*ed.*] But the sub-basement was where I lived, and I kept my nose very close to what I was doing. I did not look around Caltech much. The only things I knew about Caltech were the social friends we had, because, as I think I mentioned earlier, we were all young together. People with kids got together in the evenings. There were impromptu dinners at other people's houses. There was socialization within Kellogg because of a tradition they had. On Friday nights, they had the Kellogg seminar, and then afterward there was a party in somebody's house. It was usually at Tommy Lauritsen's house, occasionally at Willy's or Charlie Barnes's or Ralph Kavanagh's. But usually it was Tommy and Marge Lauritsen who had the party. You brought your own beer, but whoever was doing the party put out hors d'oeuvres, munchies. There was music. There were a lot of people to talk to. It was a very friendly arrangement. It was a core of the social life. You got to meet people almost immediately. Some people from around the campus came. Ricardo Gomez from high-energy physics was always there. Sometimes Bob Christy, who had originally been in Kellogg, was there. His then wife, Dagmar, did not come to very many of the parties; I only met her later at smaller gatherings. I think some of the women got tired of the Kellogg parties. They were noisy. People were drinking a bit too much—people having a very good time.

ASPATURIAN: Was there sort of a frat-house atmosphere, do you think?

TOMBRELLO: Not quite. It wasn't at that level. But it was much more of the level of a party where people were just happy—lots of conversation, lots of eating, drinking; mostly beer; a little bit of *aquavit* because of the Danish connection to the Lauritsens. I have to describe the personnel in Kellogg.

ASPATURIAN: Please.

TOMBRELLO: The person who had started it was Charlie Lauritsen. There is plenty of stuff in the Archives about Charlie. There are even books about him. He had been an electrical engineer trained in Denmark before he came to this country. He was working in a radio factory somewhere out in the Midwest when [Robert A.] Millikan [chairman of Caltech's Executive Council 1921-45] came through town and gave a public lecture. Charlie picked up stakes—wife Sigrid, son Tommy—and off they went to Pasadena, where Charlie became a grad student, a slightly older grad student, working on photoemission, I believe, which was new. The photoelectric effect was one of the reasons Einstein got the Nobel Prize. Millikan, too. Millikan got the Nobel Prize [1923] not just for measuring the electron charge but also for experimental work on the photoelectric effect, which verified some of Einstein's predictions about its being a quantum effect. So Charlie did his thesis on that topic and got his degree in rather short order [1929].

There'd been a professor occupying the High-Voltage Lab, which is now Sloan, and his name was Royal Sorensen. During the 1930s, he built some cascade transformers that would go up to almost 1 million volts. The purpose of this lab was to test components for the Hoover Dam—then the Boulder Dam—which was just being finished. They were building the electrical systems for it, and they needed high-voltage testing of the insulators and other components. Sorensen had a lot to do with that. Charlie Lauritsen saw this as an opportunity to get into a new field, which was building high-voltage X-ray tubes and getting into high-voltage X-ray therapies. Kellogg started with a donation from W. K. Kellogg, who had a ranch out in Pomona.

ASPATURIAN: This was a cereal magnate.

TOMBRELLO: That's a story in its own right, because his brother was the one who invented the cereal, but W. K. was the one who turned it into a company and made money out of it. The brother of W. K. had run a sanatorium, and one of the things you fed people were health foods, and one of the health foods was Kellogg's cereal. Kellogg was getting older and of course was getting interested in things like cancer, and so he funded the building of the Kellogg Lab, which was attached to the High-Voltage Lab. Charlie started building high-voltage X-rays, using X-ray tubes and optimizing them based on his knowledge about field emission—because one of the limitations of an X-ray tube is electrons being sucked out of the electrodes and causing problems inside the tube. Charlie had learned a lot about minimizing such effects. He was able to build some high-voltage tubes that went up to about 1 million volts. This was done in partnership with the Huntington Hospital. This is all in 1930-31 to maybe 1933; I'm not sure of the exact time sequence. A lot of things were going on.

At the same time nuclear physics was appearing. E. O. Lawrence at Berkeley had built a cyclotron and was doing nuclear physics with it. Merle Tuve had built Van de Graaffs [Van de

Graaff generators] at the Department of Terrestrial Magnetism, which is a part of the Carnegie Institution, in Washington. A few people around the country were doing nuclear physics. Charlie decided that at night, when they weren't treating experimental patients—more about that in a minute—they could accelerate positive ions and electrons on the positive and negative halves of the AC voltage cycle, so they began doing nuclear physics with these machines in the time between patients. I can't leave out Stewart Harrison, because at some point in this period of the 1930s, he was married to a woman named Katherine, nicknamed Kitty. The big gossip at the time was about Kitty running away from Stewart and marrying J. Robert Oppenheimer, and there's lots in the Archives about that, I'm sure. There's certainly a lot of literature about it. There were women on campus that everyone adored, Ruth Tolman for one. There were women that they truly all hated, and Kitty was one of the people who was not beloved by the other wives. This is something I was told by Marge Lauritsen. You'll have to look at her Archives files to see what that's about. And there are all sorts of books about that period at Caltech and later at Los Alamos.

Anyway, Stewart Harrison would occasionally come to these parties, too. When I was running the Radiation Safety Committee—now, this is jumping much farther ahead—I hired Stewart to come over as a radiation consultant for the people who were handling radiation, just thinking it would be a good idea to have a professional on the committee. I don't think they do that anymore. There are lots of things that they don't do, because people try to cut costs. I hired a radiologist, and I had an ophthalmologist who checked all the people handling radiation for cataracts every year. That physician is still practicing. His name is Ralph Riffenburgh.

ASPATURIAN: Oh, he's my eye doctor.

TOMBRELLO: He's mine, too. We're very fortunate. Ralph is still flying. He still has a plane license. He was in World War II, and he writes novels about World War II.

ASPATURIAN: So I've heard.

TOMBRELLO: He does all sort of things. He's quite an interesting man. So I used to hire him to check everybody for cataracts every year. But I'm getting away from Stewart and the parties. I'm trying to give you an idea of the variety of people who came. People would know there was a party, and they might not come to all of them, but they would drop in on some. I think occasionally Barclay and Linda Kamb came. Other people from around the campus came, but mostly on a drop-in basis. I don't think the Bachers tended to come to those parties. Though the Bachers had been certainly close to the Lauritsens when Bob was here as an NRC [National Research Council] Fellow before the war. In those days, it seemed to me that Caltech was run—and remember I was looking at this from the bottom of the totem pole at that time, so it may be an inaccurate impression—by a small group of people who'd been together for a long time. People like Bacher, DuBridge, and Charlie Lauritsen, and probably some of the people over in geology and geophysics tended to get together and decide how things were going to go. I remember very few faculty meetings in those days, either of a general sort or in the divisions. I don't think we had faculty meetings in physics, math, and astronomy probably until about the time of the Vietnam War, but I'm getting ahead of myself by just a few years.

So, getting back to my early years here, I spent most of my time doing experiments with the new tandem accelerator—spectroscopy of the light nuclei. I was getting a lot done. We were publishing a lot of papers. I was working with very good students—some of them mine, some of them borrowed from Charlie [Charles A.] Barnes [professor of physics, emeritus] and from Ralph Kavanagh [professor of physics, emeritus, d. 2010]. I think I'm going to go back and talk about the people. Willy, of course, was the great man. Willy had been moving out of nuclear physics into nuclear astrophysics, a field that he and Hoyle and the Burbidges had started back in the early 1950s with a very famous paper. [Geoffrey] Burbidge, [Margaret] Burbidge, Fowler, and [Fred] Hoyle. ["Synthesis of the Elements in Stars," *Rev. Mod. Phys.* 29:547 (1957)—*ed.*] The idea being that if you understood the nuclear physics and you had stellar models you could learn a great deal about how the elements had come into being. I think a lot of this had started with Urey at Chicago.

ASPATURIAN: Harold Urey?

TOMBRELLO: Yes. And some of the people who had worked with him in Chicago were now in the Caltech geology division: [Gerald J.] Wasserburg [MacArthur Professor of Geology and Geophysics, emeritus] had been part of that group; also Sam Epstein [Leonhard Professor of Geochemistry, emeritus, d. 2001]. We'd gotten a lot of people out of the mass spectrometry groups at Chicago, and that was part of the vision of Robert Sharp. The two people that I consider the great visionaries of that period were Robert P. Sharp [Sharp Professor of Geology; d. 2004] and Robert Bacher. When Sharp took over the geology division [in 1952], they were strong in geology, geophysics, and paleontology. They got out of paleontology—they gave the bone collection to the Los Angeles County Museum of Natural History—and he got them into planetary science and mass spectrometry, a lot of it devoted to understanding how the solar system and Earth started and evolved.

So, getting back to Kellogg, there was Charlie Barnes, who had come after the war. Also Ralph Kavanagh, who had been in the navy during the war, had come back to be a grad student here and then stayed on as a junior faculty member. He was an assistant professor when I first got here. Barnes and Whaling were associate professors.

ASPATURIAN: That would be Ward Whaling [professor of physics, emeritus]?

TOMBRELLO: Yes. Ward had been in the Signal Corps during the war. He had been an undergrad at Rice, so I sort of knew about him but had never met him. He had come here after the war. Hoyle appeared, apparently, sometime in the 1950s, with his idea that there seemed to be a gap in the nucleosynthesis sequence. There would seem to be no way to make carbon, because beryllium 8 was unstable in its ground state and broke up immediately into two helium nuclei. Therefore, you could get as far as beryllium, and then things came apart again. Hoyle had this idea that if the star were big enough, hot enough, and dense enough in its core, three alpha particles would have a chance to make carbon directly if there were a nuclear excited state at just the right energy to sort of hold them together just long enough so that they could decay to the ground state, the first excited state.

ASPATURIAN: Alpha particles being helium nuclei?

TOMBRELLO: Helium 4. Hoyle appeared with this notion. He said that since carbon exists, there must be a state of this particular property, and you should look and find it. Whaling was the one, I think with a collaborator—I can't remember the collaborator's name—who found this state exactly where Hoyle said it was. That was the first big Kellogg thing.

ASPATURIAN: When did that happen? Were you here at that time?

TOMBRELLO: No, that was before I got here. That had been worked out probably in the mid-1950s.

ASPATURIAN: So when you arrived, Kellogg must have been riding on this crest of exciting discoveries.

TOMBRELLO: Kellogg had been using a bunch of old homemade Van de Graaffs—Tommy Lauritsen had built at least two of them—and they'd just gotten a new tandem accelerator. The professors who were here were still finishing up things on the old accelerator, and I sort of jumped in and grabbed the new accelerator and all the students who'd been given projects on it. So it was an ideal time for me. I just got lucky. I got a chance to learn some of the experimental physics I hadn't had time to learn when I was a student at Rice. I got to work with some very interesting people. Bob Bacher's son, Andy, who is still one of my closest friends, was essentially my first *real* student.

Kellogg was a social club, as much as it was a research institution. You knew people pretty well. You went to parties with them. It was a relatively close group. The grad students and the postdocs tended to form a slightly separate community of their own that extended beyond Friday night. There was the pool across California Street. The kids were all in swim lessons. We would all be together at the end of the day to swim. Pick up dinners at everybody's house. Everybody had a lot of fun. It was an idyllic time for all of us, not that anyone had much money. But you didn't need much money. It was California, and life was exciting. There were so many new things going on, and any day you could come up with a new idea and just go into the lab and do it. It was quite remarkable. That was one of the reasons why, when I went to Yale, as I explained yesterday, I realized within six months that I just wasn't having as much fun. At Caltech I was having a tremendous amount of fun. Coming back to Kellogg meant I could go back and be a little kid again and play with toys. So I did.

ASPATURIAN: Who was responsible largely for the tone, that special tone in Kellogg, do you think?

TOMBRELLO: I think it must have started with Charlie Lauritsen.

ASPATURIAN: And what was he like?

TOMBRELLO: I was impressed by him. Of course, he was very smart and very quick. Very social. He tended to really run things when he was around. When I came back from Yale, I was still a postdoc, but they almost immediately started including me in the weekly meetings of the Kellogg faculty, and I think he was responsible for that. I was very pleased to be included. I remember one day I had an idea about something and I said what it was, being brash. And Willy jumped all over it. He trounced it; this was the dumbest idea he had heard of , and that all went on for a while, and then finally Charlie Lauritsen said very slowly, "Well, be that as it may, Willy, the kid is right." It was my victory. I got few victories, but that was one. But then I realized that underneath it all, the rock on which Kellogg was built was Charlie Lauritsen.

Charlie had the reputation of having built the lab. In the 1930s he had built the X-ray project. Anybody who was ever treated with cobalt-60 X rays—a low-energy gamma ray—probably owes a lot to the history of what was done in the Kellogg project. However, it was not a very successful project. They only used it to treat patients who were going to die anyway, and I suspect in many cases they died a little bit sooner, because in those days they didn't know anything about the dose regime you were supposed to have. They were learning all that, and like many cancer therapies you don't know until you've tried it whether it's actually prolonging life or shortening it a bit. But people are desperate enough that they want to find a solution, and that was one of the early X-ray therapy solutions to try to cure cancer. That continued until World War II, but once Kellogg really got into nuclear physics, they lost interest in the X-ray work, partly because medically it was not an immediate success and also because nuclear physics had a lot more promise.

Basically, when I got there, a lot of the equipment was just beginning to change. Some of it was like the stuff I'd seen at Rice—surplus stuff from the war. People had bought amplifiers and electronics that were really vintage 1940s, and they were just beginning to change that. So there was new equipment, new accelerators, and lots of bright young students, because there was money—in this case from the Office of Naval Research. So why would the ONR give money for our research? Well, I heard it this way from an admiral, who was one of their sponsors: "You know, those guys really helped us during the war. Charlie worked on the barrage rockets that the navy used. He helped to solve the torpedo problem, because the U.S. made terrible torpedoes at the beginning of the war. Charlie did a lot of stuff for us. We figured that we might have another war sometime. It'd be nice to have all these guys together, so when we needed them we could collect them all at once. It doesn't cost us very much to keep them together by funding their nuclear physics research." He says, "We don't really care about the nuclear physics research. Just having those guys where we can get at them, when we need them, is what's important."

ASPATURIAN: They wanted a brain trust.

TOMBRELLO: They did the same thing at Stanford with the electron accelerators there, which also had its own fallout in terms of applications to S band, roughly 2 gigahertz (GHz) RF technology. So we were still riding on the gratitude of the government for what had happened in World War II. Bob Bacher had run the gadget division at Los Alamos. In some sense, he was really second in command there to Oppenheimer, and the gadget division, of course, designed and built the first nuclear weapons. These were the plutonium-implosion device—Bob Christy's gadget—and the Little Boy, which was the gun-assembled U-235 bomb. And of course they also conducted the Alamogordo test.

ASPATURIAN: Did you have many dealings with Christy as a young person here?

TOMBRELLO: Sure. But Christy was moving out of nuclear physics and into developing and understanding some stellar models. The work on variable stars that he did, which was partly nuclear physics and partly stellar dynamics, made him even more famous. Of course he was around, but he was no longer part of the day-to-day operations of Kellogg. He came to seminars. He asked very insightful, very hard questions. But he was not a house theorist anymore. Kellogg was operating with Willy as kind of a house theorist in nuclear astrophysics—Willy and his guests, like Hoyle. In nuclear physics, we had random visits from Aage Bohr and at least one visit from his father, Niels Bohr, who was still alive.

ASPATURIAN: Do you remember anything about that?

TOMBRELLO: Yes. I remember that visit, and Niels Bohr had visited Rice while I was a student there. I remember he was very hard to understand, and I thought it was because he doesn't speak English, and somebody said, "No, he speaks English, he just doesn't speak very clearly. You have to listen very carefully. But it's definitely worth the trouble." He spoke slowly. I would describe it as mumbling. The son, Aage, who just died a year or so ago [September 2009], was a good deal younger. He had been with his father during the war. They'd had to get out of Denmark and were for a short time in Los Alamos together. Code name "Nick Baker," that was Niels Bohr. We had a lot of visitors. The people who had sort of started accelerator nuclear physics at the Cavendish [Laboratory at the University of Cambridge] appeared. People knew Charlie, and they knew DuBridge, and of course we had these bright people over in theory, Richard Feynman and Murray Gell-Mann, who were an attraction to all sorts of people, who came to see the wonder kids who were doing marvelous things. [Richard Chace] Tolman [professor of physical chemistry and mathematical physics, d. 1948] was dead already. [H. P.] Robertson [professor of mathematical physics] died the day I arrived, of an embolism following an automobile accident.

ASPATURIAN: That must have been a rough welcome.

TOMBRELLO: Yes. Of course I didn't know him, but to suddenly arrive on the day that somebody who is really quite famous dies— Among the mathematicians, Eric Temple Bell was gone, I believe [retired 1959, d. 1960—*ed*.]; and [Harry] Bateman was dead [d.1946]. You have plenty of stuff on Bateman and the "shoe box" file. His project was still running, though, collecting the stuff on higher transcendental functions and integral transforms. Some of the earlier people were still around: [Fritz] Oberhettinger and [Francesco G.] Tricomi, [Wilhelm] Magnus, I believe were all still here, and I probably met one of them, I can't remember which one. That was about 1962. I'd gotten interesting in scattering theory and had figured out a way to do a calculation on the old Burroughs 220 computer, which was by far the best computer I'd ever used at that point in my life. It was more advanced than the computer I'd talked my way into at Rice. Everybody knows computers are run on binary arithmetic, but the IBM 650 I had used as an undergrad ran on biquinary. It was like Roman numerals. It had a five bit, and it had four digits. It was not binary. The Burroughs 220 was a lot faster than the IBM machine I had used at Shell. It had magnetic core storage. I think it had maybe 10,000 words of storage not 2,000. It had magnetic tapes, all kinds of stuff. But it ran on binary-coded decimal language, which is 10-percent less effective in storage than binary. So the first two computers I used were not binary machines. It was only when I went to Yale that I had access to a machine—an IBM 709—that was much, much faster. All these machines were made with vacuum tubes, with all the problems that such machines have. They were down a lot. But they were very informal with the Burroughs 220 here. You went over and convinced the person who was sort of nominally in charge of it that you weren't going to break the machine, which took you about fifteen minutes, and then they wrote your name on a piece of paper that was stuck with the only piece of Scotch tape to the wall. If your name was on the list, you could come in. You could sign up for time and come over and use it. It was sufficiently slow, by modern standards—though fast by the standards of my day—that you would sit there and you could tell what part of the program was in by the pattern of the lights on the screen.

So, I would sit over there and work on stuff and then watch it. It would print out something. I would look to see what it had printed out and decide if I wanted to modify things to try something else. More and more people were learning to program, but I don't think Willy programmed himself. He had a young woman named Barbara Zimmerman, who had started as a draftsperson, doing drawings for him, and then she started learning about the computer and that was very helpful to Willy because now he basically had his own computer person.

So that was sort of the life. You fixed a lot of things. There were no service contracts on stuff. If the accelerator was broken, you went inside and tried to fix it. Depending on the level of difficulty, a faculty member might or might not be there. Electronics—well, I'd had a lot of experience at fixing electronics, but then so did some of the students. Some of the students were really good. There was a guy named [Russell] Keith Bardin, who I thought was very good at electronics. I think he had just gotten his PhD [1961] and ended up going to Columbia, and I have no idea where he is now. I've lost track of many of these people, in the mists of time, probably.

ASPATURIAN: You'd been here just a couple of years and then you were put on the tenure-track faculty. How did that come about?

TOMBRELLO: Well, it wasn't clear I was on the tenure track. They didn't have any other track. I mean, once you became an assistant professor it was not obvious you were ever going to be anything else.

ASPATURIAN: But not everyone who came as a postdoc then became an assistant professor. So, I just wondered what the circumstances of that were.

TOMBRELLO: I think the thing that impressed them— I don't know if I impressed them because I was smart; I think I impressed them because I got things done. I got *a lot* of things done. Except for Willy, I was probably publishing more than the rest of the lab. It may not have been as good as the other stuff going on, but there was certainly a lot of it. It kept the students coming out. I had a lot of students. And we had plenty of money. See, that's the reason we could have all of those visitors. Back in the middle sixties, we had over a million dollars a year from the Office of Naval Research. That would fund operating the accelerator *and* pay for visitors, postdocs, and lots of students. Now, the students weren't making very much money; and overhead was not terribly high. So the money went a long way. So we could have Fred Hoyle come in. Hans Bethe would come, and all sorts of other people would come regularly. It was really quite wonderful. Willy ran a salon. That's the only way you can describe it. It was so much fun to meet these people and hear what they had to say. A lot of them, of course, would come when springtime came to Southern California, which is about the 15th of January. And if you come from Cornell, you know perfectly well that things aren't going to thaw there for a long time. This place had an incredible attraction during the winter.

So Willy really ran something extremely interesting here. It was fun being part of it. At some point, roughly ten years later, I got to run Kellogg myself. But by that time, as I said in an earlier interview, the bloom was off. Lyndon Johnson had become president. They were fighting the Vietnam War; they were tightening up on money. It wasn't that our budgets got cut much, but while inflation, salaries, and prices of things went up, the grant did not go up. [Feynman Professor of Physics] Kip Thorne came back during that period. At first, he was sort of part of Kellogg, and then he set out to form a relativity group of his own. That was very interesting. That must have happened about 1966, something like that.

ASPATURIAN: Had Kip been one of your students?

TOMBRELLO: No. I don't remember him as an undergrad, so we may have just missed each other. [Thorne received his BS in 1962—*ed.*] He had been John Wheeler's graduate student at Princeton. Willy brought him back for absolutely the wrong reason, but it was absolutely a case of perfect serendipity. Maarten Schmidt [Moseley Professor of Astronomy] had discovered the quasi-stellar objects.

ASPATURIAN: The quasars.

TOMBRELLO: They saw these things at what turned out to be huge redshifts. At first we didn't know if they were at local or at cosmological distances. Willy thought they were supermassive stars.

ASPATURIAN: Really. I didn't know that.

TOMBRELLO: They weren't, of course—although if you start thinking about it, there was probably a period somewhere in the history of the universe where there were stars of hundreds of solar masses. They did have unusual properties. But Willy decided that you had to understand relativity if you wanted to get at these objects and understand how they generated all that energy. So he figured, OK, he knew this very bright, versatile student: "Get him back to work on that." Of course, Thorne had his own game plan. And very quickly they realized this was not likely to be the mechanism that powered quasars. In fact, I remember being at a party, where someone—I think it may have been Donald Lynden-Bell, but I'm not sure—was explaining that we're really talking about a completely different mechanism for the quasi-stellar objects, which probably turned out to be correct. But in any case, Maarten never won the Nobel Prize for discovering the redshifts of quasars. I nominated him personally for it several times. But he did win the Kavli Prize [2008], so finally there's justice in the world. Fred Kavli gave him the prize he should have won from Stockholm. It was a nice prize, and I'm very happy about that one. The original discovery happened probably about the time I left for Yale and came back—that period.

A number of things happened in that year. Gell-Mann had moved on from the Eightfold Way [a taxonomy of the elementary particles—*ed.*], and he and a former PhD student here, George Zweig, had separately come up with the idea of quarks—well, aces in the case of George. That was the big new thing—that protons and neutrons and all these other particles were made up of these fractionally charged things. Could they appear in the real world? Of course, there were a lot of experiments—not here, but in other places—looking for them, with the idea that if they're there, maybe you can see them. There was the experiment with the decay of the neutral K particles [kaons], which showed that the universe is not time-reversal invariant. The CPT [charge-parity-time] symmetry theorem may hold, but CP and T separately are broken in those cases. That was the Fitch-Cronin experiment. I remember Val Fitch coming and talking about that then.

All sorts of very interesting things were happening at that period. Everybody came to Caltech. I guess they still do, but it seemed to me that there was a lot more discretionary money, partly because of the size of the Kellogg grant. It was one of the big grants on campus. Willy could just get people to come here. It was partly to see Willy and to talk to Feynman and Gell-Mann. But partly it was just that we had money to bring them here, and at an attractive time of year. I tried to continue that when I was running Kellogg, but it got harder and harder to find the money to do it. That's why first the Fairchild money, which brought visitors in, and later the Gordon and Betty Moore funds that were used to bring in distinguished people to stay for months or even a year, was and is extremely important. It's one of the things that made Caltech extraordinarily exciting. One of the things we're going to get to when I talk about being division chair is how I looked for grants so that people would have discretionary money, lots of discretionary money, so that you'd have interesting visitors, because that is so important—not just for the people working in the field but for the students. Because I can remember meeting these kinds of people when I was a student and a postdoc, and I felt that was a huge piece of my education. It was your connection to the history of physics, and the history of how knowledge had progressed. It had progressed in such a short period of time. You got a time scale. It wasn't like looking back to Newton. It was looking at somebody who was sitting in an office. You could talk to them and you knew they had done this groundbreaking work over the last couple of decades.

ASPATURIAN: Living history.

TOMBRELLO: Caltech still gets a lot of visitors like that. The more money we can find for things like that, the more you can keep this feeling of being connected to the whole world of science.

ASPATURIAN: You seem to have become aware quite early of the need to keep a secure flow of funding going. How about your colleagues? Were they as aware of this, or was there sort of an ivory-tower attitude among some of them?

TOMBRELLO: I think there was definitely an ivory-tower attitude.

ASPATURIAN: And how did that play out?

TOMBRELLO: It played out badly for me in some ways. I felt we needed more money to support the infrastructure. Now we're jumping ahead to 1973, maybe just a little bit before that—maybe starting in 1969, with the moon landing. By now, I'm the PI on the Kellogg grant, and we're getting into analyzing lunar samples. We're also looking at radiation damage, because the reactor people wanted people who were doing things that were connected with radiation damage, and there were small amounts of money to do that. There was money from China Lake Naval Weapons Center to do some analysis on surfaces. These were projects for which people would pay to use the facilities here. The students could get involved with them, and the money could help pay some of the lab technicians and accelerator operating costs. But the work wasn't nuclear physics, and it wasn't published usually in *Phys. Rev.* [*Physical Review Letters*]. It was published other places. A lot of it was interesting science. But it was different. To be honest, I think my Kellogg colleagues would have settled for a smaller program—just done a lot less and had fewer visitors, fewer students, fewer postdocs. But Kellogg was so unusual. The other groups in physics didn't seem to have the money at that time to bring in visitors that way. For instance, the visitors in astrophysics who came through were mostly funded by Kellogg, not astronomy. Because, you see, since Caltech had always supported Palomar, Palomar didn't get much in outside funding. They were supported, and still are, largely by the general budget. It was not an entitlement program; it was kind of a payoff on an endowment for Palomar that had gotten rolled into the general budget, which at one point in the past I looked at fairly carefully to see who had come out on top in that one. But there wasn't money to bring in very many visitors. Kellogg was just unique. Willy had done a fantastic job.

ASPATURIAN: It sounds like it had a remarkable culture.

TOMBRELLO: It had a remarkable culture, and Willy always had interesting people here. Some of them were in nuclear physics, like Aage Bohr, Aage Winther, and Ben Mottelson. There was always this very strong connection to the Niels Bohr Institute. Not everyone—you know, we didn't get Herman Feshbach from MIT. There were definite pathways that worked, and some definite pathways that somehow did not get explored, but at the same time we were always doing something that was interesting. I can't take the credit for it. Willy got it started, or Charlie made sure Willy got it started. I never exactly knew how that worked. Willy was a true visionary. If there was a failure mode to the Willy paradigm, it was that Willy was the inspiration, the visionary, and one did better in Kellogg if one became one of Willy's, ah,—

ASPATURIAN: Circle?

TOMBRELLO: Minions. I remember being given a hard time for years by Gell-Mann because—and this decision had been made before my time—he was always reminding us, "You know, we kept Kavanagh and we sent Val [Valentine] Telegdi away." Telegdi was a really great physicist. He ended up in Chicago and then other places. I said, "Look, I might have made a different choice, Murray. But I wasn't here. I never had that choice. Choices got made." But Willy had a vision, and he wanted people to help him carry out the vision, and I think there were probably other models on campus like that. I believe Linus Pauling was very much the same way. Then when Linus moved on in the early sixties, first going to that Center for the Study of Democratic Institutions at Santa Barbara and then to Stanford, he left Caltech with a whole bunch of people who were in sort of postdoc jobs. Nobody knew what to do with them, and nobody knew what obligation they had to them. So this wasn't just Willy's model, it was a model that probably would not have been foreign to a German professor, and it was definitely true here. That was one of the reasons why, though I worked on Willy-type stuff, and I think I did pretty well at it, I also worked on other things that had nothing to do with that.

ASPATURIAN: You were not one of his acolytes, in other words?

TOMBRELLO: Only at times. I did experiments. I had some of my students working on some of his projects. He thought enough of me that he would send these awfully bright students, like Tom Weaver and Steve Koonin, down to work for me. They both came out of it, I think, pretty well. And I came out of it *extremely* well, working with people like that. It makes it hard to be anyplace else. But I believe there was very much this model of—not quite *Herr Doktor Professor*, but there was a definite project with a definite vision, and Willy supplied a lot of that. The students I've got now, the former undergrads who are now out looking for jobs, I tell them they have to watch out for that. You have to watch out for these personality-centric organizations where you have a lot of fun. The slogan I use is, "Watch out for Peter Pan." I think I mentioned that yesterday. You know, Never Never Land's a lot of fun, but you can end up as one of the Lost Boys. You must be very careful, because you're good enough, or potentially good enough, that you want to make a career of your own, that's got your name on it—not just further the vision of somebody else. My joke yesterday of making Formula 1's, not Chevrolets, is exactly about that. Caltech should be producing, and does produce, enormous numbers of unique individuals who go out and do what *they* think the future is. If Caltech ever stops doing that, they would be a third-rate place or a fourth- or whatever. A trade school at best.

ASPATURIAN: Hopefully that will never happen.

TOMBRELLO: I think that will never happen. There are forces in that direction, but the people we get are sufficiently tough-minded that they're going to do pretty much what they want. Now, in dealing with personalities—

ASPATURIAN: I have a question. Given Willy's model of doing things and given your own outlook and what you mentioned about Gell-Mann and the loss of Val Telegdi, was there much overt friction over any of this?

TOMBRELLO: Oh, yes, absolutely. The division was large fiefdoms. High-energy physics was dominated by Bob Walker [d.2005] and Matt Sands. And Alvin Tollestrup, who'd been a student in Kellogg, was also part of that. There also was [Valentine Professor of Physics, emeritus] Felix Boehm's group. Well, Felix plus Jesse DuMond [d. 1976]. Those were power centers within the division. There wasn't a lot else. I've mentioned Pellam, in the basement, but that was sort of a one-person group and was probably, in terms of critical mass, marginal. And so there were, if you like, three power centers in the PMA division in physics. Astronomy was—now I've really lost it. The man who ran Palomar?

ASPATURIAN: It wasn't Greenstein?

TOMBRELLO: Jesse Greenstein [d. 2002]. Jesse basically had Palomar, and ran, in a visionary way, astronomy [1948-1972]. Bob Bacher had started both high-energy physics and radio astronomy. Radio astronomy, of course, was centered at Owens Valley [Radio Observatory]. It wasn't exactly a power in its own right. I don't know if Jesse ran it or not, but it was connected in some way. I mentioned how differently appointments were handled before we established the staffing committee in 1986. Before then, groups would propose people and put them through, usually by horse trading with the other groups. Back in the early sixties, this was not hard, because there were plenty of openings. People were retiring, they were dying. PMA was building up programs, because there was a lot more funding. The place could grow. But it was not entirely collegial. [Laughter] Definitely there were old feuds, because Willy or Charlie had just pushed something through, or Jesse DuMond had pushed something through. There were hurt feelings and anger about it. You know, we were definitely divided up in many, many ways. It was not unpleasant, but it was clear that these groups were separate. That's the reason the unintended consequence of the staffing committee turned out to be *enormously* important, because it would almost have been impossible to start a new group here in the face of this polarization.

I mentioned Matt Sands. Let's talk about him. I mentioned Kellogg's old amplifiers that were sitting in the racks from the war—well, Matt had designed all that stuff. He had written the book on fast electronics or what was fast electronics during the war. And all this stuff got built. Matt had written textbooks on it and was one of our high-energy physicists. When I got here in the early sixties, he had a plan. And that was to build a big Southern California high-energy physics accelerator. I think it was going to be 200 GeV. Too big for Caltech. It would probably take getting USC and UCLA involved to make it work—and maybe other places. I think UC San Diego. Bacher decided the project was too big for Caltech, and Matt left [1963] to go to SLAC [Stanford Linear Accelerator Center]. Well, he became a professor at UC Santa Cruz, but clearly working at SLAC. Caltech decided not to grow in that direction. They were running the old synchrotron. Bacher had gotten the model magnet for the Bevatron—a proton machine—at Berkeley, and Bob Walker had come from Cornell and made it into an electron machine. He had been, I think, a grad student at Los Alamos. A lot of them knew one another from Los Alamos because they knew Feynman and Christy. Matt had certainly been involved in all that, because the fast electronics had to have been used in stuff like that. He was either on the radar project or the bomb project. So he was here. Bob Walker was nominally in charge. Alvin Tollestrup was here for a long time until he went and built, basically, the superconducting magnets that make up the Tevatron. But he'd gotten his PhD in Kellogg [1950] and was kept on. As I say, it was not so unfriendly, though there were clearly, slightly below the surface, old antagonisms. You hired Kavanagh instead of Telegdi, you know, and this was something that was never going to go away.

I see Murray [Gell-Mann] every few months now, and I am sure if we got to talking about the old days at Caltech, he'd probably mention it. "You know what Willy did!"—they all had things like that. I gather that Feynman and Gell-Mann at first had worked together and then later didn't. Different styles. The place didn't begin really to diversify in what I'd say were interesting ways until the mid to late 1980s. And I think the staffing committee had a lot to do with that. It made it also a necessity that you couldn't hire everybody you wanted to, which meant that you could make a committee like that work, because there really wasn't much alternative. If you just got into gridlock where nobody could agree on a candidate, you wouldn't get anywhere. The only exception to that had been in 1975, when we had the three powerful PMA groups that basically said, We'll each get one, and we'll get best of show for the three, and we will hire them all at once, and we will shove it down Harold Brown's throat. [See Session 2]

ASPATURIAN: So, like a museum acquiring three masterpieces at once.

TOMBRELLO: So that you don't have to choose among an old master, a Jackson Pollock and, you know—

ASPATURIAN: —an Impressionist.

TOMBRELLO: The full spectrum. You get your Monet. Everybody gets something. But you have to be careful, because, you know, art museums that aren't willing to diversify get into trouble. This is a good analogy for universities. I remember looking at the art of a friend of mine who used to run the Pasadena Art Museum before Norton Simon acquired it. Bill Agee went on to run the art museum in Houston, where they have a huge collection of Frederic

Remington bronzes but not much else. Now, the obvious thing when somebody like Agee comes is, "Why don't we sell some of the Remingtons and get a more balanced collection?" And they went crazy. They could not deal with that. They had a hard time accepting the idea that they had this wonderful collection, but it was just one thing. It was just one bronze after another of cowboys and Indians. I love Remingtons, but when you have a room full of them, they all sort of look alike.

Universities can be that way. They will have one strong group and they can't get around it. They don't realize that unless that strong group gets weaker somehow—or smaller anyway, it doesn't necessarily have to get weaker—the rest of the department's not going to change. You find that at universities that have, by one piece of luck, hired a star, or a person who became a star, and are then unable to get past it—to grow anything else. Caltech could have almost done that in physics.

ASPATURIAN: What do you think saved it?

TOMBRELLO: I'd love to think it was my staffing committee.

ASPATURIAN: Which was initiated when, again?

TOMBRELLO: 1986. There was a general agreement that we should do something, even before that, but we never seemed to get anywhere. Gell-Mann always called solid-state physics "squalid-state physics." He realized there were bright people in the field; he just didn't want to hire any of them. It was hard. You get into gridlock; it's hard to get out unless some of the cars just go away. By 1986, clearly, Feynman was two years away from dying. It was the right time. It was hard to get appointments through; you could only get them one at a time. LIGO, you see, got started with essentially no faculty participation except Thorne, and then you can read in my Archives LIGO oral history about how Robbie [Vogt] got chosen to head LIGO. But there are still relatively few professors attached to LIGO. [Barry] Barish [Linde Professor of Physics, emeritus] came in sort of part-time to be director. It's a great thing to have happened for LIGO. Robbie had run to the end of his tether and couldn't go on any further. They needed somebody who would pick it up from there. Robbie had done an enormous amount—they wouldn't exist if it hadn't been for Robbie. But he'd gotten to the point where somebody had to carry the project through to completion, and Barish was that person. But [Alan] Weinstein [professor of physics] has moved over now, probably more or less full-time, from high-energy physics. [Ken] Libbrecht is there at least part-time. I think I had something to do with talking Ken into doing that, in fact. I was chair when we hired the one person in LIGO who's really in LIGO, and that's [professor of physics] Rana Adhikari. They had never really had a LIGO professor before. Then of course, there's the whole story of LIGO and how things got crosswise with the rest of the institution because Ron [Ronald W. P.] Drever [professor of physics, emeritus] was brought in.

Yes, that was a special committee. Frank Sciulli and Kip Thorne and I, and—I can't remember, probably Barish—were a committee to make a recommendation to hire somebody in gravitational physics. We hired Ron Drever, who for some years was only half time here because he was still trying to keep his gravitational-wave program alive back in Glasgow, partly because he felt responsible to the people there. So, in fact, Adhikari was not the first gravitational radiation person—Drever was. That was in the late 1970s.

ASPATURIAN: This reminds me I'd like to go back to something you said a little earlier about how physics in those days was really a collection of fiefdoms. What was the role of the division chairs? Did they set a tone for the division?

TOMBRELLO: Well, with Bacher, I suspect Bacher set the rules down. I don't think anybody argued with Bob Bacher. He had been there when the place *really* became a different place.

ASPATURIAN: Now, did he take over from Carl Anderson?

TOMBRELLO: No, Carl took over from him [1962-70].

ASPATURIAN: OK.

TOMBRELLO: But you see Bacher had become provost, so I think I'm quoting Carl accurately that, "Well, he's over there in Throop, but he's still trying to run physics." And I think he did, to some extent. Carl was smart. He was good, but my guess is he was still pretty much, if not told what to do, strongly advised by Bacher about how things would run. Then [Robert] Leighton replaced Anderson [1970-75]. Leighton, of course, had been here since he was a grad student.

He'd done all kinds of things. He started in cosmic ray physics with Millikan or [H. Victor] Neher, probably both. He'd done theoretical work with Houston, the guy I mentioned earlier who went off to be the president of Rice. He had done all the nice stuff on the magnetic fields on the sun. He had gotten this photography mission on the approach to Mars [the *Mariner* missions]—these beautiful pictures of Mars as the spacecraft got closer and closer. I guess at that point he was designing these telescopes that are still the best millimeter and sub-millimeter dishes in the world, both in Owens Valley and on Mauna Kea. Leighton had done everything. This is the universal man, and he was a nice man, an extraordinary man. I hope his story is in the Archives—

ASPATURIAN: It is. I did it.

TOMBRELLO: —because it's a wonderful story. Wonderful man. But he discovered that if you try to make a change, say in high-energy physics, you became very— He was a one-term chair, and it was not clear he wanted to be a one-term chair. I think he had stepped on some toes and—

ASPATURIAN: What happened?

TOMBRELLO: Well, they just looked for another chair.

ASPATURIAN: You mean, he ran up against some difficulties?

TOMBRELLO: Yes. And he didn't give up easily. Bob was really very tough, and very smart. But, you know, you have to have the consent of the governed. It's not that he was thrown out, but it was clear he should not try for another few years as chair. Anderson had been in eight or nine years. Bacher had been in from '49 to '62. Leighton was in five years. I was on the committee that picked Maarten Schmidt as the next chairman. Maarten tried something different, and I have to give him credit for trying. He tried to create a council of the senior people, and because at that point I was running Kellogg, I was on this thing. It was not the friendliest of operations, but later I understood what he was trying to do. He was trying to get the strong voices in a room to settle things among themselves, so that the faculty meetings—by then we were having real faculty meetings—would be more collegial. Of course, that is exactly what I did with the staffing committee. I put all the strong voices in the division on the staffing committee. We would sit out there and decide whether we were going to have the kumquat or the pomegranate or the apple or the orange. It was not always totally a happy choice. But by the time you made the choice, everybody went into the next faculty meeting marching in step, and that had a big effect. I think that's what Maarten was trying to do. But what happened to Maarten was that while he was chair, the divorce with Carnegie occurred, and in response to that he took over the directorship of Palomar and Robbie Vogt became PMA division chairman [1978-83].

ASPATURIAN: The divorce with the Carnegie Institution—

TOMBRELLO: Yes. We shared the facilities. Basically, they had Mount Wilson. They had Cerro Tololo [in Chile]. And we had Palomar, but it was all run as one thing.

ASPATURIAN: Was there a particular reason that it separated?

TOMBRELLO: Ah—

ASPATURIAN: If this isn't going off on a tangent.

TOMBRELLO: We wanted to hire a woman as a professor. I think that just did not fly at Carnegie. They didn't like her. I will not mention who she is. This was a strong personality, a leader. She's now at Santa Cruz. That was part of it. It also could have been a build-up over time of a lot of little things. The thing that brings things to a separation is not necessarily the real cause. It may have been a lot of causes, and I don't know what they were. All I know is, by the fall of 1979 we were in the middle of a divorce settlement with Carnegie. Maarten then took over Palomar, Robbie became PMA chairman, and that was an interesting period. You've read my analysis of Robbie's personality in the Archives. Robbie is a genius. He was one of the true visionaries of modern-day Caltech—that is, 95 percent of the time. The other 5 percent of the time, I have explained, I think, candidly in the other part of the Archive. Robbie was just doing fine with the administration until the trustees' chair, Rube [Ruben] Mettler, saw the other 5 percent. And when the chairman of the trustees saw what the rest of us had seen—this other side, with totally irascible behavior—I believe it then became a risk-avoidance issue for the trustees. That's when Robbie was fired as provost [1987]. It all happened very fast. What the administration and the trustees did then was absolutely brilliant. They brought in somebody who was so identified with Caltech, and his wife was so identified with Caltech: Barclay and Linda Kamb. [Linda Kamb is Linus Pauling's daughter.—*ed*.] He just calmed everything down.

I remember there were a bunch of events on campus the night after his appointment. We were at one of them. Barclay and Linda made sure they hit every event. I remember that as they came past us Stephanie said, "Linda and Barclay, what do you think this is? Camelot?" But it was. This golden couple who are so identified with Caltech that the reaction just was, "Everything's going to be all right." They did something absolutely brilliant to put Barclay *and* Linda in.

ASPATURIAN: Was there a precipitating incident with Dr. Vogt?

TOMBRELLO: Well, certainly, for all of us who had seen Robbie in his rug-chewing fits. Robbie would get extremely angry. It was impossible to deal with him. I'd seen him do that with Murph, who was president, and Murph stayed calm through it all. But it wasn't, I think, until Mettler saw Robbie like this that something was done. As I say, Robbie's a genius. He's brilliant. He's visionary, and he had some great ideas; but there was that 5 percent of the time. You wonder if you could take the risk on something like that, and I believe the trustees—the trustees were terribly divided. There were some people who wanted to keep Robbie and fire Murph. I'm sure there was a spectrum of opinion.

Where should we go now? We've almost gotten to 1982, 1983, when my divorce from Kellogg occurred and after that—after I was booted out—Koonin and Barnes took over.

ASPATURIAN: Under what circumstances were you, as you say, booted out?

TOMBRELLO: Well—

ASPATURIAN: If you want to talk about it.

TOMBRELLO: I was asked if I would step down. I lost a vote of confidence, shall we say, with Willy and Gerry Wasserburg. To be blunt, the Kellogg grant got into financial trouble. We had gotten cut. They had given us a raise, which we'd had for a couple of years.

ASPATURIAN: This was the NSF?

TOMBRELLO: Yup. And then they cut it. Suddenly, we are in a leveraged position, and I ask everybody what they could do to spend less money. And Wasserburg had been getting maybe $200,000 a year, something like that. I basically assessed everybody with a 10-percent cut. And Gerry just could not accept going from $220,000 to $200,000. Burnett [Donald Burnett, professor of nuclear geochemistry, emeritus] was cut out entirely. He had more to complain about. He'd been probably getting $30,000 or $40,000. It was a bit of an empire. We tried to integrate intellectually the stuff that Wasserburg and Burnett were doing with the nuclear astrophysics, because it did fit. But that's how I lost Wasserburg. And Willy just wanted everything to go to nuclear astrophysics, and he couldn't understand why I just didn't move the money over from some of these other grants that supported doing radiation damage and lunar samples and stuff. I just wanted to get this thing down to a level of where we could support what we had, and it was going to be, hopefully, a short-term thing.

But anyway, there were people who were unhappy. I have my own management style. It's not necessarily quite democratic. I consider democracy a spice. I mean I consider it a basic ingredient. I think it makes things work better, and I think, used sparingly, it's nice. The rest of the time, somebody's actually got to make decisions and not just vote on things or ask opinions. I'm not sure I asked anybody's opinion about hiring Koonin, but I think it was a good move. [Laughter] It's worked out well. It was that kind of thing. After I left, and I described what happened in an earlier interview [Session 2], Koonin and Barnes tried to run Kellogg, and their styles were just so different. I think Koonin basically said that he could not work with Wasserburg and Barnes. And so suddenly the group did not have Wasserburg in it, and it did not have Barnes running it. Koonin took over the group. Then things in Kellogg began to take on a completely different direction, going more into high-energy nuclear physics. Koonin was putting his stamp on it. By then Willy had won the Nobel Prize [1983].

ASPATURIAN: So we're in the mid-1980s now.

TOMBRELLO: Yes. It took roughly two or three years for Koonin to reorganize things. Willy was not threatened by it. Willy felt—I'm putting words in his mouth, because I saw the way it was handled—that there would probably be no new appointments in nuclear astrophysics, but that the people working in it, like Barnes and Kavanagh, could continue to work in it. That's actually a very good solution. Whereas Koonin then would take some of the new people I hired—like professor of physics Robert McKeown—and they would go off in a different direction. I think that has been reasonably successful, though Kellogg is down to maybe one person now, it may just be [professor of physics Bradley] Filippone, because it's not clear that Bob McKeown is coming back from Jefferson Lab. It's a high-energy electron accelerator in Newport News, Virginia. He's not the director of it, but he's director of something, some piece of it—maybe director of research or something. [Dr. McKeown is deputy director of science at Jefferson Lab—*ed.*] Nuclear physics at Caltech used to have a lot of professors, and now it has one. But then again, from then on, Kellogg didn't have a big vision. When Koonin became provost [1995], you know, they really lost the visionary. Bob's a very fine physicist. So is Filippone. But Koonin had a much bigger vision and played on a much bigger international stage. Then he became provost and was playing on the stage for the entire institute.

ASPATURIAN: So we're now up to about '86 or so, and I think '87 is when you went to Schlumberger–Doll.

TOMBRELLO: Right.

ASPATURIAN: Let's take a break..

THOMAS A. TOMBRELLO

SESSION 5

December 27, 2010

TOMBRELLO: OK. We've gotten down to the point at which I've left Caltech for Schlumberger research lab, and we talked yesterday about some of the underpinnings of that, the preamble to that. As I said then, I had been a consultant there since '81. By 1986 the lab had been cut back dramatically. But they had a lot of people and projects still rattling around, and so they brought me in on a two-year contract to see if I could straighten it out.

ASPATURIAN: So you took a leave of absence from Caltech?

TOMBRELLO: I took a leave of absence. Yes, perhaps a little bit about the negotiations for it.

ASPATURIAN: Sure.

TOMBRELLO: The person who recruited me had become Schlumberger chairman at the end of 1986, so he was a new boy, too. But he had been with the company quite a while. His name was Euan Baird. I met him in '81; I was having dinner with three people and realized that maybe one of them, or more than one, would be chairman of the corporation. One of them—Michel Vaillaud—did become chairman and lasted a year, because he couldn't get rid of Fairchild Semiconductor, which they had bought after the people who founded it had moved on to found their own little company, which they called Intel. I had advised against buying it; I said they'd lose $400 million, because that was the purchase price—and, well, I made a mistake. They lost $2.5 billion before they realized they were not made to run a semiconductor company.

So they bought a distressed property, shall we say, and then ran it into the ground. But, anyway, Baird got to be chairman after one year of Vaillaud, and he and I had met once to talk about how the lab was doing. We met at the Union Club in New York. He wanted me to be candid about the director they had brought in from Exxon. I was slightly less candid than usual, but basically I said, "The guy's not doing a very good job, and the people don't respect him." He wouldn't have asked me to come and meet with him if he hadn't already made the same decision. It was just putting a dot over the "i."

Euan's a very bright man. When he became chairman, he said something that people should have taken more seriously, which was, "If I'm still in this job in ten years, you people have made a dreadful mistake." He stayed in the job fifteen years, and he bought his own version of Fairchild and lost probably $5 billion. Being in a job too long—and I'm speaking for term limits here, because they exist for the reason that when you're in a big job for a long time, you're cut off from your sources of real information. People don't tell you the truth anymore, and if they do, you don't like hearing it and you don't believe it. Euan was absolutely right, except when his time came, after ten years, he still thought he had more time going.

But anyway, I was given a chance to go there, and I was negotiating. I flew up to Palo Alto, because Michel Guilloud, who had run the lab when I first started consulting there, was staying up there for something or other. Baird came in and we met to talk about the details of running the lab. They wanted to cut the lab's funding back. In 1985, 1986, they had been spending probably $42 million a year on it. He said, "I suppose you have an idea of what you want for a budget." I said, "Euan, I'll tell you something. I will make you an offer you can't refuse. You will tell me how much you're willing to spend to run research. And then I will tell you what I can *give* you for that much money." I said, "That's the best I can do. You probably have an idea in your mind about how much you want to spend, and then I will tell you what I can do with it. However, when we agree on a number, it is absolutely fixed. You cannot start playing around with the number. After six months you cannot tell me, "Oh, I'd like to cut you back some more." He says, "Oh, this is the most refreshing thing I've ever heard in my life. It's a pleasure to do business with you." I said, "Keep in mind there's a caveat. You can't play with the number. You get to choose it—it's your number—but then it's *my* money." And I said, "I'm not going to try to do this in a vacuum. You've been in this field longer than I have. You and I will negotiate on what the mix of projects will be and that kind of thing. We are going to make sure we agree on what we do, but we're not going to change the money." And so he says, "$30 million."

ASPATURIAN: Just like that?

TOMBRELLO: Yup. Oh, it's wonderful to work for a company that is very flat, and people can make decisions very quickly. The upside is, you can make a decision and you can get on with it. The downside is, you can make a decision and get on with it in the wrong direction. [Laughter] They were capable, and many people are capable—maybe like the George W. Bush administration—of making big decisions based on not enough data and going off in the wrong direction.

Anyway, so we do that. I go in and discover, of course, that nothing has changed in roughly the year since they made the personnel cuts. They have far too many programs. They have people who are spending most of the time on their computers polishing their CVs, because they are looking for other jobs. And I go in and start trimming. People who have been running a program suddenly discover they are worker bees instead of leader bees. One department in the lab had nineteen projects, and at the end of my second day there, they had two projects.

ASPATURIAN: It took you two days to make this determination?

TOMBRELLO: You had to. First, you have to make progress on these. Several of the projects were things we call case studies. For an industrial research lab, they are wonderful, absolutely brilliant. You get yourself into a limited partnership agreement—not in the legal sense, but partnering with one of your clients, like Shell or Unocal or Elf, or one of the big oil companies. You agree that you can show them proprietary technology that's in the research stage, not in production, and they can't talk about it. You give them a deal on that. It's a shared-cost thing and a shared risk. And now you have the funding for some scientific development study. It's great for the research lab, because they get to see what it's really like to be out doing something in the field. It's great for the company, because they're seeing stuff years early. It hasn't already been engineered, so you're in a position to get and run the best data you can.

For example, one of the things I started during that period was a project in the Middle East on the nature of shale source rocks. We did this way back in '88. Today, that rock has come to be economically very interesting in the United States, and the reason you can buy natural gas for $4 per million cubic feet is that some of these source rocks contain a lot of gas that hasn't gotten out. They are tight shales, and they contain a lot of gas. But at the time it was something really new. We didn't know what the consequence would be, that it would make the

U.S. independent of foreign natural gas probably for a *very* long time, but that's what came out of that project.

There were many other projects of different sorts. There was stuff on improving the resolution on seismic profiling out in the North Sea. There was stuff on measuring porosity of oil saturation of stuff, probably also in the North Sea, with Elf. Projects like these are great. They give a touch of reality to research. They give a touch of advertising, as in "You like it? Hey, we can put that thing in production." We had engineered something called a dipole sonic tool, basically setting up shear waves in the soft formations, which propagate more slowly than the compression waves. Before that, everybody had been doing basically compressional waves sonic, where basically you have a sound source in a well. The waves go out, and if they hit the right structures, they come back into receivers, also in the well. You get much higher resolution close to the well. The shear thing allowed us to work in some formations in the Gulf Coast where other kinds of techniques did not work well. There was a lot of that kind of thing. And remember, these projects had been contracts. The previous director had gotten himself into a financial hole, which was not entirely his fault. It *was* his fault that he didn't practice triage and kill off a bunch of essentially extraneous projects, because when you have a contract with, say, an oil company or companies, you damn well better fulfill that contract. I discovered there were about three of these contracted projects where people were making no forward progress, and we just had to kill a lot of other stuff to make sure those things got finished.

ASPATURIAN: How did you determine what should stay and what should go? I mean, it sounds as if you were expected to do it very fast.

TOMBRELLO: Well, it wasn't that fast. I started the negotiations on the job sometime in the spring of '87, and I wasn't going to take over until June 1. The guy who was going to be my boss basically said, "There is a secretary there—an assistant—who will give you anything you ask for. Her name is Bari Ross Perlman." I knew her because I had been working forty days a year in the lab for several years as a consultant. She just sent me everything. I read very fast, and I got through a lot of material. I just read pounds, all kinds of stuff. I said to Stephanie, "You know, I need to know more about her, because she is the odds-on choice to be my assistant when I go there, but I don't know anything about her." Stephanie says, "If she's as good as you say she is, she's going to find a way to tell you." And so in this mountain of stuff that came to me, there are a couple of pages that she put together, "in case you're curious about me." It's a little CV and a short bio, and, oh yeah, she found a way to tell me. Before I got there, I'd actually done a bit of work, and I made sure she was going to be my assistant.

I knew I had to fire the head of one of the departments. I couldn't do it until I got there, but I was already beginning to audition people. You would approach somebody you thought might be a candidate for the job and say, "Write me a visionary statement of what this department should do under the present circumstances." There was a bit of that going on, too. I had a bit of luck that the chief financial officer was a woman I knew, Ellen Burns. She was going to be leaving the company to go into business for herself. She was an accountant, married to one of the senior scientists, whom I think she met at the lab. She was extremely helpful, because she told me where certain bodies were buried that were useful to know about. There were lots of things about the lab that did not appear on pieces of paper. There were mysteries, but she was quite willing to be candid about answering why these mysteries had occurred. So I had gotten a few months' head start, even though officially nobody knew I was going to be running the lab—but since I was a consultant, I could get a lot of information from people. I could talk to them. I think only one person figured it out, and he figured it out about a day before the announcement.

The job paid well, shall we say. It paid nicely. And it was a great education. It was an education in, first, being responsible for a lab. You paid the taxes. You paid the electrical bills. You had some dealings with the New York office, but you wrote the checks. If they got over a million dollars, you had to get some big vice president to countersign it, but, you know, a million dollars is a lot of money. They were very careful about how many people you could have on staff. They did want to keep some of the number of full-time equivalent employees. But that was not a terrible task to do. They didn't micromanage at that point—which you can take as a hint that maybe they started micromanaging later.

Our lab had to build some sort of relationship with the rest of the company, because Schlumberger may do research, but they're a service company. They have an operating arm out in the oil patch. They do things for money. They make measurements in oil wells. They do seismic profiling. They're a service business, and research is a tiny little piece pasted on top of all that. That's the reason the case studies were important. They created a relationship with your own operating people and of course a relationship with the clients. It's hard running a research lab without knowing what the clients want.

ASPATURIAN: No kidding.

TOMBRELLO: And you don't always get the correct information about that in quite the right form when it comes in through the people who are working in operations.

So, it was fun. There were challenges. There were a few people in the lab who were troublemakers, but on a percentage basis very few. I would sometimes get sort of bent out of shape, because I had a couple of people who were real pains. Then I started thinking: I've got 200 people in the lab and if I have two people who are difficult, that's not a bad percentage for a human population. This is fantastic. These are some of the nicest people I've ever known. I'd been told it was a cosmopolitan crowd, and they were. They spoke languages from everywhere. They came from everywhere, but mostly young. They mostly wanted to have fun. They liked doing research. They worked *all* the time. It was great. Since Stephanie didn't come back East to Connecticut with me—Kerstin was in senior year of high school, and she didn't want to move—I would come back every couple weeks for a long weekend. I still had ten grad students back here, too.

ASPATURIAN: How did Schlumberger compare to the environment at Caltech? Similarities; differences?

TOMBRELLO: Bright people, eager. The equivalent of the students or postdocs around you—that part was similar. Politics was different, in that you were dealing with a very thin upper management, which meant they were close to you. It was not unusual for the chairman, when he had a bone to pick with me, to tell me to come in to the city and hear what I was doing wrong, which was interesting.

The politics were very different. There were people who tried to take advantage of the fact that they didn't think I understood the culture. I saw an interesting example of that. When I was negotiating for the job, I realized there was a market research group in my lab. But they didn't report to me. So I told the VP who hired me—we ruined a perfectly good dinner with excellent wine in this negotiation—"If they report to me, I'll pay 'em. If they don't report to me, you pay 'em." He said, "What do you know about marketing?" I said, "I have a feeling I may know as much as you do." [Laughter] Which is a great place to start.

He had a strong personality. He was perfectly willing to deal with controversy. His name was Andre Salaber. He was a Basque. He used to say, "You must think you're tough." I said, "Yeah, you Basques think you're tough, too." We had agreed that night that he would pay them. Well, I got there in June, and sometime during the winter—the snow was falling—Mario, one of the vice presidents, roughly at my level, shows up and says, "We've changed our mind. You're going to pay them." I said, "Oh, so they're going to report to me." "Well, no." And I said, "Oh, no?" I said, "I had a deal." "Well, we've changed the deal." I said, "Yeah, you've changed the deal." I stick my head out of the office, and I say to my secretary, "Bari, I want you to get John Roddy up here." He was sort of my building person. He comes up. I say, "John. You know Mario. He's got this group here that does market research. You know where they are. They're all together, which should make this easy. I want you to take all their stuff, put it in cardboard boxes and I want them out just off the edge of the property. And I want it done quickly." Mario, the vice president, says, "It's snowing." I say, "What does that have to do with it?" He says, "You wouldn't do it." I said, "John, would I do it?" He says, "Yeah, boss, you'll do it." I said, "OK, when I give you the word, it's going to occur very quickly. I want all those people and all their stuff out there, off the edge of the property, by the side of the road. And Mario will tell you who's going to come pick them up." Mario says, "You'll do it." I said, "You better believe it." He makes a phone call back to Andre and says, "I think we don't have a deal." [Laughter]

So then I'm at some retreat they're having for those of us who were sort of high middle–level VPs, and someone—the general counsel, I think—came up to me and said, "You know, you've only worked for this company for six months. Why is it you play the game just the way we do?" I said, "You want to play this game? I can play this game. You want to play tough guy—'I can make you do what I want,' and all that? I can play that game with you. But you know, at the end of the day, it doesn't make any of us a penny." I said, "Why don't we just forget about doing this kind of stuff and worry about making money?" And he looks at me as if I've come from Mars, because everybody knows business is not about making money. For the people doing it, it's about power. It's entirely about power. It has nothing to do with making money, most of the time. I just had the wrong notion of why people in business do things, particularly in companies like that. No, it wasn't about money; it was about showing who was boss. Well, I won that particular battle. I didn't win all the battles. I won that one.

ASPATURIAN: You weren't bluffing.

TOMBRELLO: Oh, no. Oh, no. You don't dare bluff playing with people like that unless you're just willing to leave. You have to be credible, and they respected that. It didn't mean that things like that didn't continue to occur. It meant that they knew it was just too much trouble to deal with some loony who was going to put this department—you know there were probably ten people in it—out by the side of the road.

ASPATURIAN: In the snow.

TOMBRELLO: In the snow.

ASPATURIAN: Quick sidebar: Do you play poker?

TOMBRELLO: No. I don't play games very well. [Laughter]

ASPATURIAN: OK. Carry on.

TOMBRELLO: No, but I read—I guess it was in *The Economist*—that Obama may well be a chess player, but he's sure not a poker player.

ASPATURIAN: Lyndon Johnson was a poker player.

TOMBRELLO: Yes! Not my favorite president, but he certainly knew how to play the game. And Clinton knew how to play the game—different style of poker player, but he knew how to play the game. Astute politicians. And there's always some of that at universities. It doesn't dignify the operation. It doesn't make anybody any money. It doesn't make operating things any simpler. But playing the role of Dirty Harry is something you sometimes have to do. And I guess I don't mind doing it.

ASPATURIAN: Evidently not.

TOMBRELLO: But at the end of the day, as I say, it doesn't mean you've really accomplished anything. So, anyway, Schlumberger. They began to monkey with the budget a little bit after the first year, and I complained. They didn't really cut it. They may have cut it a half a percent. [Laughter] I mean, compared to the fluctuations in federal grants at universities, it was noise. But I had to put up a show of saying how disappointed I was in them—how this was unfair, this wasn't part of the agreement. They felt they had to do it. OK, they got away with it. But then, by somewhere in the middle of the second year, they began to dabble a bit more. And then they said, "Are you going to leave us?" I said, "I haven't decided yet." "Well, when are you going to tell us?" I said, "When I get ready." [Laughter] I said, "I have a contract, you know." So eventually, I did decide. I had learned a lot of things from the experience. Actually, I missed being at Caltech. I missed the students.

I had the equivalent of students at Schlumberger. I have a story about that. The previous director had done something brilliant. He hired a guy named Bob Burridge, an applied mathematician from Courant Institute at NYU. I asked Burridge why he had come to Schlumberger, and he said, "You know, I never really had many students at Courant, and there are all these young people here in the lab." Then he said, "I'm in the geophysics department, but I'd really like to report to you." I said, "Well, Bob, you're probably one of the most senior and distinguished scientists here. I could put up with that, but there's a price." He said, "Well, what's the price?" I said, "Ah, the price is the following: I may pick out some young scientist in the lab. And I will say, 'Bob, I'd like you to work with that person for maybe six months, on a problem of your choosing, his choosing. You work together.'" He says, "Hey, that sounds wonderful to me. Not much of a price." I said, "At the end of the six months, I'm going to have you come in and talk to me very candidly about how this person is doing, what their promise is, the rest of it." He says, "Well, that's what professors do." I said, "Fine; I'll arrange it today that you're reporting to me. I want you to keep your office, because you're an asset to the geophysics department, but at the same time you're reporting to me, and remember the price."

Then I would find somebody who I thought was bright enough but who I had a feeling would not necessarily have a long-term future, and who was going to hit a ceiling at some point. And so I'd say, "Bob, why don't you work with Jorge?" "Oh, yeah, he's an interesting guy."

Six months later, “OK, Bob. Did you have fun?” “Oh, yeah, smart guy. We worked on this, this, and this.” I say, “Wow, that’s great stuff. How would you compare him to this other guy you’re working with?” “Oh, he’s not in the same league,” I’d get him to explain that. “Very good, Bob, thank you. I’ll give you another assignment soon.” I’d bring Jorge in and say, “Look, Jorge, no one’s going to fire you. But my projection is, you are not going to be promoted to the next level of scientist. You’re not going to become a senior scientist.” There’d be the usual—

ASPATURIAN: Angst.

TOMBRELLO: Yes, but nothing dramatic. In the case of Jorge, he said, “OK, I understand what you’re saying. You’re saying I’ve got a future here, but it’s a limited future.” He said, “Let me think about it, because I may come in with a proposal.” The proposal he came in with was, “I’m going to take a sabbatical. I’ve found a place in South America that would like me to come for a year. Would you pay for it?” I said, “Under what conditions?” And he says, “If it works, I’m not coming back.” I said, “It’s a deal.” And so I adopted my own tenure-review system. I used Mr. Bob Burridge and a couple of others as my test committee. It’s not that any of these people were bad. It’s just that I felt that I had to keep trading up, and I had to have a way of moving people out of the lab if they hit a plateau that I didn’t think they could get over. I could have been wrong about it. People do restart themselves. There were some people I tried to restart, sometimes successfully. But universities do have certain strategies that industry does not always understand. It’s different from a university, but there are things that transfer, particularly to a research lab. If you’re out there in the field, I guess it’s the same thing. It’s a different set of talents, but there are people who are going to make the grade, people who are someday going to be candidates for chairmen. Our chairman, Euan Baird, though he was a Cambridge grad, had been out in the field, out in the Middle East, and earned his spurs. Not only was he smart about a lot of things in science, he had actually been out there dealing with the real world.

People are people. It’s just that you’re evaluating different skills and different talents. The last thing you want is an organization where everybody looks alike. You want a diversity of intellectual approaches to tough problems, because you never know what you’re going to run into.

ASPATURIAN: It's true, but you know, it takes an intellectually confident leader to adopt that approach and be comfortable with it.

TOMBRELLO: It's not just intellectual confidence. It's partly a willingness to get beyond stereotypes. Stereotypes can be useful ways of categorizing things, and universities have stereotypes. Some of the people who do not get tenure here have probably not gotten tenure because they don't look quite like the rest of us.

Certainly, people moving up the ladder in Schlumberger have much more of the Dirty Harry personality—at times, they have to. They also have to have a few people who are a little quiet. There was a person there named Chad Deaton, who didn't quite make the cut. He was put in competition with a guy who is the current chairman at Schlumberger, and lost. But he now runs Baker Hughes, and he's doing a great job as chairman. He may not have fit Schlumberger, but he fits Baker Hughes really well. There was a guy in computing, whose name I can't quite remember anymore, who got turfed out of Schlumberger. He ended up being head-to-head with Carly Fiorina to run Hewlett-Packard. Lost that particular battle, but it was close. He went somewhere else. You know, maybe he didn't fit Schlumberger, or maybe they just adopted a stereotype that wasn't complete enough. Because both these guys ended up doing really well in their next jobs. I think you have to be careful of stereotypes. Very careful among students, too, because the students come in all sort of flavors.

ASPATURIAN: Actually, that reminds me: With regard to Caltech faculty and PMA, were there people here who were lost who should not have been? In your view?

TOMBRELLO: I'm not sure. I'm not at all sure. Most people who deserve it get tenured at Caltech. There are very few cases where I believe it's just so clear that you have made a terrible mistake. We put a lot of effort into hiring the right people the first time. And then we go through the reappointment process after three years, and even that's a halfway step toward tenure.

One thing I did as chair, which I actually copied from the Biology Division and modified, was to create a tracking committee. I didn't want people getting lost. Every new non-tenured faculty member got assigned a three-person tracking committee that is to meet with them, answer their questions, make suggestions, write a little report at least once a year—more often, if necessary—that was sent to me. Then I bring the person in, give them a copy of the report, and we talk about how they're doing. The tracking committee is composed of the executive officer for that option in the division—physics, math, or astronomy. There's a person *in* this person's field; there's a person in a related, not exactly the same, field. The idea is that they're supposed to give advice. They're supposed to give feedback to the chair. And they're supposed to not let this person get lost. I think it's been a help. I don't know that it will change the success ratio, but I think it also means that you should have fewer recriminations at the end about whether this person has been evaluated carefully. Then, when tenure comes, if they get to tenure—or reappointment and then tenure—the tracking committee forms a kind of a nucleus of the reappointment or tenure committee. There are at least several people on the final committee who have been with this person the whole time they've been at Caltech. They're supposed to be a fan club, but also a *critical* fan club. I try very hard not to get a group that's just out there to try to get rid of the candidates.

It's interesting which questions come up, because they're not always questions about research. In fact the most obvious thing that comes up with research is that the person is trying to do too much, and they're told to cut back., focus on a few things and get them done. But many of the questions that come up have to do with mortgages and Caltech's assistance with mortgage subsidies.

ASPATURIAN: How about child care?

TOMBRELLO: Oh, child care! My wife, Stephanie, is one of the founding mothers of the child development center here. I don't know if they still call it child development.

ASPATURIAN: The Children's Center at Caltech.

TOMBRELLO: Children's Center. You realize it was not popular when it started, in the early 1970s.

ASPATURIAN: That doesn't surprise me at all.

TOMBRELLO: They really put almost no money in it. They had to give them space, but they charged them for it; they didn't like it. The women here who were in the previous generation didn't understand the point of it, because they didn't work and they didn't understand the whole model. My first wife, Ann, was a relatively young wife of a senior professor, and so she was caught right in the middle. She had gone back to school. Now her kids were old enough that she didn't need that kind of child care, but she *understood* what this new group of women were about, and why they didn't want afternoon meetings of the Caltech Women's Club—because they were *working*. It was an interesting transition. Stephanie was one of the activists. I didn't know her until later. But Ann was allied with that side in a way, trying to translate what they were saying to the women that *she* had been associating with and who just didn't understand why these women were being so difficult about something that they hadn't needed at all. The answer is, Well, they need it now.

ASPATURIAN: Was there anything particular about your two years at Schlumberger that you brought back to Caltech that affected or reinforced your outlook or shaped your thinking or behavior?

TOMBRELLO: Well, it convinced me that I really could run something. Of course, I had been running Kellogg for a while—roughly nine, ten years—so I knew I could run something. But this was more freestanding. This was something where I was the one who sent the Christmas presents to the local police department and the fire department and, you know, dealt with the local taxes and the rest of it. That's a confidence builder. You can take a complex situation, break it down into pieces, and make it work. You also have more scope than I had at Caltech—though I did run Kellogg and that was not a small operation. There may have been, at the high point, ninety people associated with Kellogg—students, faculty, and technicians and engineers. Budget of several million dollars. Now, it was a much smaller budget than at Schlumberger, but a lot of them were students, and therefore they weren't paid very well.

Yes, I think it was confidence building—the feeling that you'll make mistakes and you have to figure out ways to correct the mistakes. Of course, you always try to avoid making mistakes, but you can never avoid making them. You often have to make decisions based on limited data. You also learn to evaluate personality types that can either help you or that will make things harder. Another thing you learn is that the tendency to consider too many alternatives for too long costs you money. That is not a bad idea at the beginning stage, where you have two approaches to some project you're trying to do. But at some point you've got to choose one approach because you don't have the resources to do the project twice. I still see them trying to do too much at once at Schlumberger. The current management of this lab—which they moved from Ridgefield, Connecticut, to Cambridge, Massachusetts, which in some ways is a big improvement, not being out in the boonies or the suburbs—still tends to keep several projects going at once, when they can really only afford one. It is much better to have a critical mass in favor of one solution and to arrive at your choice from some rational basis. You may be wrong, but when you're keeping a bunch of things in progress, you have to make that decision. And learning that made a huge difference, because I started a bunch of big projects while I was [division] chair.

ASPATURIAN: I have one more question for this session. This is out of left field, but you mentioned that when you were talking to one of your counterparts at Schlumberger who was a Basque, he said, "Oh, do you have to be so Sicilian, you think you're so tough," or something like this. It made me think—I believe it was in the early 1970s that Mario Puzo wrote *The Godfather*?

TOMBRELLO: Well, I read it in paperback in 1970.

ASPATURIAN: OK. This was of course followed in quick succession by those two superb Coppola movies. Did this put you in a new light vis-à-vis some of your—I mean, suddenly, here is a new iconic presence on the American scene.

TOMBRELLO: Well, it certainly created—

ASPATURIAN: You know what I'm saying.

TOMBRELLO: Yeah, well, the [Joe] Valachi tapes and things like the Kefauver committee in Congress had occurred a generation earlier. My grandfather always refused to believe any of it, or at least refused to admit that any of it could be true. *The Godfather* made me appreciate my grandfather and what he had had to face in coming to this country, and that, just like Vito Corleone, he got pushed into a position of authority. Because he had to survive. He had a family. I began to understand that my grandfather had basically one choice, which was to live. And the question was, What rules are there? How do you survive in this society that doesn't care about you at all? One thing in the book that just hit me was the scene where the Don dies. He's out there playing in the garden with the little kid, his grandson. I thought, Oh, my God! My grandfather had retired. He was working down on a river in Alabama, where basically he was taking care of somebody else's home and fishing camp. He had a garden there. I remember being down there as a little kid and going through the garden with him: "Taste this, Tommy. Taste this. Try this. Try a tomato. Taste this; it's dill. This is oregano!" I thought, Oh, my God! Oh, my God! There's this little kid in this book, and he's having exactly the same experience with his retired grandfather, who's a nice old man in the twilight of his life. Then, immediately thinking back, Oh, yes, but he wasn't always like that, he didn't just get to this nice retirement.

There was an interesting experience down there at the river. Once, when my uncles were there, the man who had hired my grandfather said something as if he was kind of disciplining him for something. I don't know why my grandfather chose to work for this guy rather than just buy his own camp—but he did. But my uncles explained—quietly—to the owner of the property what an honor it was that my grandfather had chosen to live there and take care of his place. I think there was no more trouble after that. So there were small similarities.

ASPATURIAN: Some resonances for you.

TOMBRELLO: A few resonances. [Laughter]

ASPATURIAN: I wondered if the whole Godfather thing perhaps resulted in some of your colleagues or people who met you saying, Oh, well—.

TOMBRELLO: Oh, that's kind of a joke.

ASPATURIAN: Yes, exactly.

TOMBRELLO: It really is a joke. As I was saying earlier, I think that my mother, who was of German descent, a little hill-country girl from central Texas, was probably at least as tough as my grandfather and wasn't afraid to show it. That generation really had to deal with a lot of bad things. I think my generation has had less to deal with. I was lucky. I was too young for World War II. Too young for Korea, too old for Vietnam. I just skated through there. Born in the middle of the Depression but growing up basically in the boom times after World War II, easier times. We weren't the greatest generation. I hope we do our best.

ASPATURIAN: Maybe the luckiest generation.

TOMBRELLO: Maybe we were the luckiest generation. I don't think it's the high point of American society, I really don't. I look at the kids who come out of Caltech and say, "Oh, they're just as bright as anybody who has ever lived in the history of the world. They're going to change things." Actually, that's a good place to stop.

THOMAS A. TOMBRELLO

SESSION 6

December 28, 2010

ASPATURIAN: We continue today with Professor Tom Tombrello, who has been at Caltech for—

TOMBRELLO: Almost fifty years.

ASPATURIAN: —almost fifty years, and he is going to talk now about some of the many interesting and unique personalities he has encountered in his years here. Does that sound like a fair description?

TOMBRELLO: Yes, I think so. One name that occurred to me today, because I was reading an article on old manuscripts in the new *Economist,* is John Benton, professor of history—

ASPATURIAN: In the Division of Humanities and Social Sciences.

TOMBRELLO: Right. Wife Elspeth Benton, who was the first director of the Children's Center at Caltech. I did not know John very well until I was on the President's Fund committee, which dispenses small amounts of money for joint research between Caltech and JPL [Jet Propulsion Laboratory]. This is probably getting on toward thirty years ago. We tended to get proposals to do things in technology, science, and engineering. There would be a PI from Caltech and a PI from JPL. Typically these were grants of, at most, $50,000—the money came out of the management contract that Caltech gets for running JPL. You don't often get proposals from a professor of history, but we got this proposal from John Benton. He wrote in and said professors of history want to go look at old manuscripts in person, because you can see things in the original that you couldn't see if you saw just a photograph. But they don't have very much money. He said he wanted to use some of the new image-enhancement techniques—remember, this is now thirty years ago—that JPL was developing and apply them to manuscripts so that scholars who can't afford to travel where these manuscripts are can get a facsimile of the manuscript that shows things like erasures. We were—the committee was—just swept away by it. It was clearly an idea whose time had come. When we told Harold Brown that we were going to fund it, Harold basically said, "Well, of course. I'm glad you came to that conclusion, because if you hadn't, I would have anyway, because I like that proposal." That showed a lot about Harold. But John was very successful with that. In fact, the images you got were far better than looking at the original manuscript. You could see all kinds of things. John was a very, very interesting man. He had very severe arthritis and died just over twenty years ago, in a fall in his house. One of the great losses to Caltech. He was a case where you could see how someone in the humanities could benefit from being at an institute of technology. I always had hoped there would be other people in the humanities who would find things where an appropriate use of science would make their field stronger. So, that's the first person I was thinking of. Do you have somebody particular in mind?

ASPATURIAN: I'd like to come back to something you said yesterday, when you spoke about Bob Bacher and Bob Sharp. Since they sort of encompass a lot of institute culture, let's look at them.

TOMBRELLO: Very different personalities. They both did something big. Bob Bacher came here in the late forties, a little after Lee DuBridge did. Funding was shifting from the big foundations, like the Rockefeller Foundation funding Palomar, to the federal government. That was a big, big change. Bacher and DuBridge changed the character of Caltech. People who think Caltech has always been one sort of school are wrong. It has had many face changes—

ASPATURIAN: That's a neat way to put it.

TOMBRELLO: The late 1940s were when we moved onto the stage of doing things that had importance in Washington, and, as I think I've said before, in those days we also had the gratitude of Washington, because the scientists' contribution to the war effort had been enormous. For whatever reason, keeping the groups of scientists together or rewarding them—whatever it was—was extremely important, at least until about 1968. So Bacher built high-energy physics and radio astronomy here, two fields that were important.

ASPATURIAN: What was Bacher like as a personality?

TOMBRELLO: A very strong personality. Though I loved the man, he did not listen. He'd lecture. Everyone remembers going in and being told things by Robert Bacher, and you'd better listen and you'd better do them. But on the whole, he had extraordinary judgment, was an extraordinarily interesting man. We all knew he had done something big during the war. Running the gadget division at Los Alamos was important. He was clearly second-in-command to Oppenheimer. I never knew Oppenheimer. I met him, but I never knew him. We'll talk about Oppenheimer next, after we get through with two of my heroes, Bacher and Sharp.

Sharp took over a very good but narrowly directed program—geology, geophysics, paleontology, and bones basically, and of course seismology. [Beno] Gutenberg [professor of geophysics, d.1960] and [Charles] Richter [professor of seismology, d.1985], you know—they were highly successful. Sharp saw to it that the bones got given away or sold to the L.A. County Museum of Natural History. Seismology, of course, would continue, because Richter and Gutenberg were still here. But Sharp was the one who started basically the mass-spectrometry research as it is applied to meteorites and planetary samples of various kinds, and the work on lead in the environment by Clair Patterson. He got the geology division into planetary stuff, because JPL was now beginning to do things that Sharp could see were going to be important. He and Bacher had extremely different styles. Bacher knew where he was going and you'd better go along with it. I don't mean that in a negative sense. He was determined. Sharp was also determined. Sharp knew exactly where he was going, but there was a good-ol'-boy style to it. I grew up in the Deep South, so I know about good-ol'-boy styles, and you should be careful when you see it in people, particularly politicians. I never saw it applied to a division chairman before. You should watch out for people who pretend to be a good ol' boy; they're trying to convince you of something, and you're going to be led astray because you think this person is more limited than they really are.

ASPATURIAN: Can you give me an example?

TOMBRELLO: There was a person down in Livingston, Louisiana, and I cannot remember his name. He was in the House of Representatives from Livingston. I was down there for, I guess, the dedication of the LIGO site. He got up and gave a talk. He started off in a good-ol'-boy style, which was basically telling Cajun stories. And right in the middle, basically to show off a little bit—he had his law degree from Harvard, though he may have had his undergraduate degree from Louisiana—he switched. Suddenly, the accent was gone and he was pure Harvard Law. I don't think he winked at us, but intellectually he winked at us—basically showing us he could work either side of the street. You wanted Harvard Law? Without the Southern accent? He could do that. You wanted good ol' boy who told Cajun stories? He could do that. Whatever it took.

ASPATURIAN: And Sharp was kind of like this?

TOMBRELLO: Sharp was like that. But Sharp was wise and strategic, incredibly strategic. There was clearly some competition with Bacher. I remember being told—I think by Barclay Kamb—that both he and Ron [Ronald Lee] Shreve were students in physics here as undergrads. Sharp got them away from Bacher as grad students. Shreve did his PhD work [1959] on the Blackhawk landslide, and Barclay, of course, went into glaciology. It's very interesting that Sharp really knew people. He was not the least bit soft, but it didn't show. People *loved* Bob Sharp.

ASPATURIAN: And they highly respected Bob Bacher, from what you're telling me.

TOMBRELLO: Eventually you respected both of them. But Bacher had a very different style. Effective, but not as sneaky as Sharp. Sharp was brilliant and clearly one of the people I just adored at Caltech.

ASPATURIAN: It sounds like it.

TOMBRELLO: He was wonderful. We used to go out to lunch. He always ate at the cafeteria that's downtown, just off Lake.

ASPATURIAN: Beadle's?

TOMBRELLO: Beadle's.

ASPATURIAN: I don't think it exists anymore.

TOMBRELLO: He had limited taste in fine food. We went to a cafeteria, which of course I knew well, because growing up in Texas you got used to eating in cafeterias.

On to Oppenheimer. It was amazing that [General Leslie R.] Groves picked him to run Los Alamos. His first choice was Gregory Breit. I knew Gregory, because later on I was at Yale with him. He would have been very compartmentalized, very controlling. I suspect it would have been much more like the German bomb project and probably just about as unsuccessful. Oppenheimer was charismatic.

ASPATURIAN: Apparently.

TOMBRELLO: He won people over. He had many enemies, because he could also have a very hard sarcastic edge and often used it on people he didn't respect. Made a lot of enemies, and that was part of his eventual undoing. But, to just talk about the Los Alamos thing, my take on Oppenheimer was that he was the world's greatest project manager. At Los Alamos, he had a bunch of the world's great prima donnas. Somehow he kept all of them moving forward, charming and inspiring them all. How he did it was something I was very curious about, because before I did my LIGO oral history for the Archives I was looking into how LIGO had run and thinking about the problems they got into. Any time you have a big complicated project, you wonder how people generate wild cards. By "wild card," I mean an alternative that you have on hand in case something really hits an obstacle you can't get over. So I was very curious about Oppenheimer and Los Alamos, because somewhere, probably in 1944, they ran into an obstacle they hadn't anticipated. Their U-235 bomb design had pretty well worked out. It was gun-assembled, basically. You take two subcritical pieces of uranium-235, bring them together rapidly, they reach critical mass and explode. They thought they understood that. With plutonium-239, they anticipated they had a problem. But they underestimated it. When you make plutonium-239, which is the analog of uranium-235, you get a certain number of the heavy isotopes of plutonium, like plutonium-240. Those fission spontaneously. The trouble is that you can't bring the pieces together slowly, because these heavy isotopes will give you a fizzle, where the whole thing heats up and disperses its energy before you get to critical size. So they had designed a gun-assembled weapon for plutonium-239 and, remember, their first samples of plutonium came from the Lawrence cyclotron at Berkeley. Since it was basically made by protons, you didn't get much neutron-rich stuff. They designed this bomb, which had its own problems, but it was, again, gun-assembled. It was longer. You needed higher velocity because of the plutonium-240, but they figured they could get around it. There were some other problems with the aerodynamic stability of this thing, which was called Thin Man. It tended to rotate in a plane rather than falling like an arrow. But they figured they could work *that* out. But when they got the first plutonium samples from the Hanford reactor, it had more 240 than the previous sample from Berkeley, and they knew they couldn't assemble a gun to make critical mass. So almost immediately, they jumped into an implosion design, where you take something that's roughly spherical and you compress it with high explosives so it becomes a smaller sphere, reaching critical mass *that* way.

ASPATURIAN: Seth Neddermeyer.

TOMBRELLO: I'm going to tell that story. I knew Seth.

ASPATURIAN: You knew him?

TOMBRELLO: Yes. I spent the summer once at the University of Washington, where he was at the time. But my Los Alamos question was, When did Oppenheimer start the implosion project? Because almost overnight, after they got those plutonium samples, they were doing implosion. And the answer was, Probably about day one. Seth Neddermeyer had this idea of implosion and had been given a tiny little room with, I think, five people to study implosion and do experiments—not very successful experiments, but doing them. That's when I realized Oppenheimer was such a brilliant project manager. He anticipated a possible obstacle and he started working on it *early* in the project—not when he hit the obstacle but long before. Now, the interesting part of the story, from Neddermeyer's point of view, and which Neddermeyer never quite liked, was that once Oppenheimer saw that implosion might be a solution to the critical-mass problem, that project went from five people to five hundred in a couple of days. Neddermeyer became an advisor to George Kistiakowsky, who was appointed to run it. Here Oppenheimer again showed the strength of a project manager, realizing that the person you had for the wild card was not necessarily the person you needed to implement his idea. That was truly brilliant.

And that, of course, showed up in the LIGO project. Robbie Vogt guided LIGO through building a very successful prototype and achieving a design, but it was Barry Barish who carried the project through to completion. The whole LIGO story, again, shows how important it is that you have a project director who has, first, the vision to know you need something extra and the will to change the project's course and change the people running it when you have to.

Now, there's another interesting Oppenheimer story about another little Los Alamos project that started on almost day one. He had Edward Teller—a group of one, because Edward couldn't work with anybody else—who was very interested in fusion bombs. As I said in something I wrote, it didn't pay off at the time, but you might say it represented a move in the direction of a totally new product line that had considerable significance in its own right. It's a bit pedantic to say it that way, but it was true. Out of that little one-person wild-card project grew a whole other direction in nuclear weapons. I think that booting Teller out of the fission-weapons group, while keeping him on and keeping him thinking about this, shows Oppenheimer's wisdom, too. Someday somebody is going to write a book about Oppenheimer as a project manager. I tried to convince Jeremy Bernstein to do it.

ASPATURIAN: Jeremy Bernstein being *The New Yorker* writer for physics.

TOMBRELLO: He did write a book about Oppenheimer [*Oppenheimer: Portrait of an Enigma* (2004)] , but he was tantalized by the possible romance between Oppenheimer and Ruth Tolman. Jeremy and I were corresponding, because he wanted me to find out, Was it true? Some other author had published a book with that in it. I said, "Well, I'll look into it." So I talked to Margie Lauritsen [widow of Tommy Lauritsen]. And Margie's first reaction was, "Nothing to it. I knew Ruth very well. Never happened." So I got back to Jeremy and said, "This is what I learned, but you have to be careful. Everyone adored Ruth Tolman and would probably do anything to protect her reputation." But with Kitty Oppenheimer, they didn't care, and she didn't have any reputation as far as they were concerned. I said, "You don't have very much information about the Ruth Tolman thing. You have a somewhat ambiguous, flowery letter she wrote to Oppenheimer. You also have to fold in the fact that people tended to write a lot of letters in those days, and the style of writing them could be a little bit over the top by present standards.

Did she have an affair with Robert Oppenheimer? You may never find out for sure. Certainly, no one who was there—none of the women there—are likely to tell you."

ASPATURIAN: While we're on the subject of titans of 20th-century theoretical physics, in these oral histories there's always the inevitable question about Richard Feynman. Or if you have anything you want to say about both Feynman and Gell-Mann, I tend to think of the two of them both together and in counterpoint.

TOMBRELLO: Oh, let's start with Feynman. I have to say, I did not know Richard well. I was his colleague for many years. Knew him, talked to him, but never worked with him. But you have to keep something in mind about Richard Feynman. Richard Feynman was always, to some extent, performing. He was always on stage. And part of this, I think, was a way of preserving a certain amount of privacy. He was such an attractive figure that, I believe, to generate any sort of private life he kept people at a distance by wearing, effectively, a mask. That came home very strongly when he died and there was a memorial service at Caltech. I remember sitting there thinking, None of these people really knew this man. He was always on stage to some extent—I don't mean that in a negative way. He was an attractive man. He saw the world from a vantage point that few people ever reached. Many, many times. I mean the stuff on liquid helium shows that. The stuff on the Feynman diagrams shows it. Truly one of the most original people at Caltech.

There were two extremely original people I knew at Caltech—neither very well, but I did know them and talk to them. The other one was Fritz Zwicky. Zwicky did all sorts of things. Zwicky was, of course, the man who discovered dark matter, because he looked at the rotation curves of galaxies and said there has to be something there that we don't see or Kepler's laws are wrong. Of course, that led to a lot of studies of that, and now to one of the great mysteries of this century that we hope to solve. But he had a very different memorial service. Zwicky was very irascible, far more irascible in public than Feynman. It was interesting to watch Zwicky at somebody else's seminar, when he pointed out to them that he'd done that work twenty years ago and they hadn't quoted him. But at his memorial service, suddenly a side of Zwicky appeared that I think few of us knew anything about. After World War II, he knew that a lot of the scientific libraries in Europe had been destroyed. And he set out on a one-man crusade to collect books, personally box them up, and send them to libraries in Europe to replace the lost books. In the Feynman memorial service, I don't think there was a huge amount of emotion, because I think people had always been kept slightly at a distance from Dick. With Zwicky, there weren't very many dry eyes in the house. We were seeing that a man who everyone thought was an ogre had a side that nobody had ever seen. It was very, very different. It was so striking. We suddenly saw a Zwicky who certainly wasn't obvious in public or in his dealings with the rest of us.

Gell-Mann. Oh, yes! I knew Gell-Mann better. I still see Gell-Mann occasionally. I guess I saw him a few weeks ago. He's gotten interested in cancer and is working with a project I'm associated with, down at USC. It's a physical sciences approach to cancer, funded by the National Cancer Institute. Murray and I are kind of wild cards in that. Murray is much more overtly mathematical than Feynman. I remember Murray once saying, "Feynman is always looking to see where the gears connect, and there aren't any gears. There are just these fundamental mathematical symmetries." Well, they're both right. There's two ways of looking at the world.

Murray could also be very difficult in public. I have a couple of stories. Well, the standard one is of Murray coming to a seminar he didn't like and just sitting in the front row noisily reading the *New York Times*. That was hard to deal with. But I can remember that when I came here as a postdoc, in 1961, I decided to sit in on Murray's class on—well, it amounted to field theory, but it was on whatever Murray wanted to talk about. He looked out at this first class. Everybody wanted to be there, and there were probably a hundred people at that point. He looked at them and just shook his head. "This will never do. The textbook is [Silvan] Schweber's book." [*An Introduction to Relativistic Quantum Field Theory*]. This was a new book, about two inches thick, on very, very fine, very thin paper. So it had a lot of pages, and the pages contained few words and lots of equations. It was probably not $100, but for that day and time, it was expensive. Next class is still pretty large. We all appear with the book, and he says, "Oh, OK," and thumbs through this book. He says, "Simple stuff. Read the first half for the next class." Well, the next class, there are many fewer of us. He looks out at us and says, "Hmm. Are there any questions?" Questions? We can barely lift it, much less read it all. He says, "Ah, good. I knew it was simple stuff. Finish the book for the next class." Well, you could have had the next class in a phone booth, and some of us actually tried to ask some questions. But Murray, being Murray, got up and said, "Good. I'm glad that's over with. Now we're going to talk about what *I'm* interested in, which is Regge poles." And we took off on Regge poles. I never thought about the book very often after that. Some years later, when I was running the research lab for Schlumberger, somebody said, "Could you get Gell-Mann to come here and give a seminar?" I said, "Sure. But why would you want to?" "Well, he's a great man." I said, "True. But he'll come here and insult you." "We don't mind," they said. "We don't mind." So we got Murray there, and in introducing him I told that book story. He looks at me and says, "I didn't do it." I said, "You did do it." He said, "Well, maybe I did it." [Laughter]

Murray was a showman, and he baited us sometimes. There was one class where he was deriving something. We'd gone back to how you turn Feynman diagrams into integrals—because that's what they are. They're a type of shorthand for writing down a certain set of integrals, which gives you the probability of that particular reaction occurring. Murray is at the board, dropping all the constants. *Pi*'s have disappeared. 2's have disappeared. Velocity of light has been set to 1; *e* has been set to 1. At the end of it, somebody—probably Eric Adelberger, who was, I believe, a second-, maybe third-year graduate student [PhD 1967]—sarcastically said, "You can't calculate with it, Murray. It doesn't have any of the constants in front of it." Well, Murray turns around slowly and *sneers* at us—Murray can sneer—and says, "You want *numbers*." "We want numbers." And he says, "I'll do it by dimensional analysis." I laughed. Everyone laughed, because how can you get four *pi*'s with dimensional analysis? Maybe *e* and *c* you can get, but you're not going to get four *pi*'s. So Murray races through this with a set of arguments that no one can follow. At the end of it, there is not only the integral but there are all these numbers in front of it. Well, nobody *dares* challenge it. But we write it down *carefully*. Go home and of course every one of those four *pi*'s and whatever were there and in the correct place. I am convinced he set us up, but I cannot prove it. It was—

ASPATURIAN: A *tour de force*.

TOMBRELLO: A *tour de force*, any way you describe it. It was an interesting class. You felt physics was being created before your very eyes. We would go home after the class and try to figure out if we could do something with it. It was magnificent.

ASPATURIAN: You were a postdoc at this time?

TOMBRELLO: I was a postdoc. I was sitting next to Carl Anderson in the back row, who was trying to figure out if he could learn something from sitting in on Murray's class. [Laughter] I think we were both baffled most of the time. But then most of the students were, too. Murray is a showman. Dick was a showman.

ASPATURIAN: Very different.

TOMBRELLO: With Dick, there was the Feynman effect. It's like the Chinese restaurant effect—ten minutes after dinner you're hungry again. With Dick, the lecture was so clear that you quit taking notes. And then five minutes after the lecture, you couldn't reproduce the lecture! I remember when Matt Sands and Leighton, people like that, were taking notes for the Feynman lectures in freshman physics. They often realized at the end of a talk that they couldn't reproduce it. They had photographs of the board. They had recorded what Feynman said. Still, there was something elusive about it. I'm not saying it was wrong or incomplete. It was subtle. And you didn't realize the subtlety, because it was so smooth, it was so beautifully done. It was a piece of artwork. But you had to constantly be aware of the fact that because Dick made it seem so simple, you were missing key things. The Feynman effect. It was very interesting.

ASPATURIAN: Well, let's see. We've covered three of the four Nobel laureates in the division during your time here: Fowler, Gell-Mann, Feynman. While we're talking about it, let's go to the fourth—Politzer. He won in 2004 for asymptotic freedom.

TOMBRELLO: Well, David. I love David. David's quite an extraordinary person.

ASPATURIAN: You've known him since he came.

TOMBRELLO: I've known him since he came. He was part of that 1974-75 triumvirate we hired. [See also Sessions 2 and 4.] Three of the brightest human beings in the known world that year—certainly in science. David is interesting. There are people who say he's only done one thing, but they are all desperately envious and they wish they had done that one thing. He's a marvelous teacher—has a marvelous sense of humor. Plays stringed instruments very well—banjos and things like that. I remember that in 2004, the year he won the Nobel Prize, by a quirk I found out who was going to win the prize about a month early.

ASPATURIAN: How did that come about?

TOMBRELLO: I shouldn't say.

ASPATURIAN: Oh, but you will.

TOMBRELLO: It wasn't from the Nobel committee. Someone let something drop. I happened to interpret it correctly, let's say. Someone who had reason to know what the physics committee had been doing and said something about people I might have nominated in the past. And I thought, "I have just gotten a hint that it's going to be somebody who has been in the queue a long time." I didn't think it was Maarten Schmidt, though I'd nominated Maarten. I thought, "I bet it's Politzer." But I knew—knowing David—that David might choose not to be available for public viewing after he won.

ASPATURIAN: Why did you feel this?

TOMBRELLO: Politzer and I used to see each other at the gym every day, and I told him, walking back from the gym, "You know, you're going to win this year." He said, "I've heard that before." I said, "Yeah, but you're going to win this year." And I got the feeling from his answer that he just might not be around for the interviews and the Champagne party. I looked up when this was going to be, and I realized I was going to be in the United Kingdom doing some stuff for Schlumberger, and I thought, OK, I'm division chair. We've got to wire this so there's no embarrassment for the division. David can do what he likes, but I have to have this covered. So I went to John Preskill [Feynman Professor of Theoretical Physics] and said, "He's going to win this year, John. You have to be ready and have something carefully written in advance that we give to the reporters, and you have to be ready to get up and talk about it in public instead of David, if he isn't around." We covered it. I said, "He's going to disappear. But if he doesn't, we're covered. If he *does*, you're going to do it for him." I told David Baltimore. Baltimore just did not believe it. He didn't believe I knew, because the controls on that thing are very, very tight. But I knew I knew. I interpreted something that was a hint dropped deliberately, accidentally, I don't know. But the name was never mentioned. It was a small detective story that I interpreted correctly. David didn't believe it, but I knew it was going to happen.

So there I am on the day; I'm riding in a car. Someone had arranged for me to be driven from Cambridge, U.K., where Schlumberger has one lab, to Abingdon, where they have another. In the middle of this, a phone call comes through to this car, from Bob O'Rourke, who was running public relations at Caltech. O'Rourke is furious at Politzer. I said, "Well. You know he's the one who's won the prize, and he can decide what he does. It's his call. It's not your call, Bob. It's not my call." I said, "I've left you covered. If I had not left you covered with plenty of written stuff you can hand out, and a perfectly adequate, very interesting speaker named John Preskill, then you'd have reason to complain. But you don't have any reason to complain. Politzer won. If he decides he doesn't want to be part of this right now, that's his call." He was not happy. Jane Dietrich, one of your colleagues [editor of *E&S* magazine, 1986-2004], was very unhappy with Politzer and was, I thought, negative about it. I tried to tell her she was off base. She hadn't won the prize. There was no reason Politzer couldn't do exactly what he wanted, having won the prize. He's a nice person. He's done an enormous number of things for the students, particularly the students of Caltech. And for Caltech. And, by God, he won the Nobel Prize—hey, let him call it any way he likes. I said I would be willing to bet I could get him to come to a celebration of this prize, and he did. Everybody said, "You guys have a lot of fun." We made up songs. Politzer played the banjo. Preskill and I talked. It turned out to be a real love-in for Politzer, and he deserved it.

ASPATURIAN: At the press conference, though, Mark Wise [McCone Professor of High Energy Physics] ended up doing the honors, not John Preskill. What happened?

TOMBRELLO: I don't know, because I was in the U.K. But Preskill had written enough stuff that Mark Wise could pick it up and take it. We were covered no matter who stepped up to do it. Politzer wasn't answering the phone for a few days. But I was amazed at the reaction and how people got upset with him about it. It's his Nobel Prize. He can do what he wants. It's clear he deserved it.

ASPATURIAN: What I had heard, from somebody who knew somebody who knew somebody else, was that someone on the committee that decides these things had such terrible feelings against David Gross that he said, "David Gross will never win the Nobel Prize as long as I am alive." So when the word came down that the three of them—Wilczek, Gross, and Politzer—were sharing it, I wondered whether this individual had passed on, or whether this was just all purely apocryphal.

TOMBRELLO: I don't know. Things like that occur. There was the book written by Diana Buchwald—Diana Barkan when she wrote the book—about [Walther] Nernst [*Walther Nernst and the Transition to Modern Physical Science* (1999)]. Nernst, I believe, ended up winning the chemistry prize because he was blackballed on the physics prize. So Nernst had two pathways to success. Whereas Arnold Sommerfeld—he's the kind of scientist I mentioned earlier who has a legacy of generations of successful students—had a blackball against him in the community. And since he was only in physics, it held, and he never won the Nobel Prize. I gather that these things happen; I don't know for a fact. One thing I *do* know has happened in current years—I will try to not be too specific about it. I think the committees try to do a very, very careful job of sorting out who should win—who'd done the work and what were the circumstances. They even commission—I've been party to that—various people who might at some future point be candidates and get them to write a personal history of what they did. I was told explicitly they were not to be modest. But they were to be accurate. I collected several of those and passed them on. However, for any given Nobel right now, there are three prizes to be given. Not four. Not more. Occasionally one person, like Zewail [1999], wins, but never more than three. So if there are fields where there are legitimate claims for more than three people, there's an awful lot of vicious infighting out in the community to push particular candidates forward and to push other candidates down. This is one of the least attractive things about the current Nobel situation. I do not believe that extends back to the committees. I think they try to do a very good job. At least, from what I know about it. Though it's not to say there aren't people on there that say things. I don't know if anybody said that about David Gross or not, but it is certainly possible that somebody would take a dislike to someone or for reasons that may actually have something to do with the science. Gross and Wilczek published, basically, a paper that was right next to the one that David Politzer sent in. So, you know, it was clear it was a horse race to the finish, and they both had gotten the answer. Whether [Gerard] ’t Hooft had done the work earlier is a more interesting matter. Because I think he asked a very careful question when he won, a few years earlier [1999], of *exactly* what had he gotten the prize for and realized that the committee had left an opening for honoring the discovery of asymptotic freedom as a separate prize. ’T Hooft certainly deserved his Nobel too. The question is whether he had claims that were not being considered because they knew they had another prize. That I don’t know. But I did know his reaction to it, which was, What exactly did I win it for? How broad were the claims? They had left an opening, and we knew it at the time. It had left an opening and it might be a future prize.

ASPATURIAN: If you had to handicap future Nobelists out of the division, do you have any?

TOMBRELLO: I had one and he committed suicide this year: Andrew Lange. I think he certainly was one of those people who went to the next stage in the interpretation of anisotropies in the Big Bang radiation.

ASPATURIAN: That was a major breakthrough.

TOMBRELLO: That was a major breakthrough. He was the first to get real numbers out of it with the BOOMERanG [Balloon Observations Of Millimetric Extragalactic Radiation and Geophysics] experiment. BOOMERanG, of course, was a work of genius in its own right, because it was a cheap way to build a satellite that went around the Earth; it merely orbited the South Pole, as a balloon. But he got there before WMAP [Wilkinson Microwave Anisotropy Probe] by years, and you might say skimmed the cream off what WMAP has done. They’ve done a marvelous job with WMAP, but Lange got there first with a very clever experiment. That’s solved one of the potential political problems of where to assign the credit—Lange is not here to get the prize anymore. It’s very sad. If I had to guess, and this is nothing but guesswork, I think some of the things that Jeff Kimble has done in quantum communication have been quite remarkable and very interesting. If something happens with LIGO detecting gravitational radiation, I believe you can begin trying to figure out who might be on the prize, but Kip Thorne has to be there.

ASPATURIAN: That would be a very difficult one to parcel out, I think.

TOMBRELLO: Except I think it's very clear Thorne's going to be on it. The others I'll not speculate about. But there might be others at Caltech. Certainly, Thorne himself has been the driving force in that whole thing. But again, it's a crapshoot, because they haven't detected gravitational radiation. [Russell A.] Hulse and [Joseph H.] Taylor [1993 Nobel laureates in physics] made the discovery of the indirect effect of gravitational radiation some years ago now by looking at the orbital decay of binary pulsars, binary neutron stars. It was a brilliant piece of work but not a direct observation; it was more calorimetry. It said that the binary system is losing energy at a rate consistent with gravitational radiation. They studied it for a very long time to prove that. So in some sense, there has already been a prize for the discovery of gravitational radiation. But the direct detection, as in the case of the direct detection of the neutrino, is an important thing that still has to be done. It could be done tomorrow. I was hoping it would occur while I was division chair, but it didn't happen. Though it might take years before the prize actually is given. Because that's what happened with the Politzer prize.

ASPATURIAN: Thirty years.

TOMBRELLO: That's right. In the case of my brother-in-law [Robert C. Merton], who won it in economics with Myron Scholes, it took twenty-four years, because the first papers, the Black-Scholes paper and then Bob's paper, which was back-to-back with it, came out in 1973. The history of that was a bit weird, too, because Bob didn't try to publish his paper, because he knew that Myron and Fischer [Black] had actually come up with an important part of the idea first and their paper had been rejected for publication. It was only later, when their paper was published, that they put the two right together and published them. That was certainly a gentlemanly approach to it. The sad thing was that Fischer Black died of cancer beforehand, or it would have been a three-way prize.

ASPATURIAN: That is sad.

TOMBRELLO: Certainly, Fischer Black was a very interesting man. There's a quotation of his that's absolutely perfect and that lots of modern-day economists, including some that advised the government, should think clearly about. He had taken a job at, I believe, Goldman Sachs, having been a professor at MIT [Sloan School of Management]. Anyway, somebody said, "Well, what's the difference between being at a university and being on Wall Street?" He says, "Well, market efficiency looks a lot different from the banks of the Charles than it does from the banks of the Hudson." That's a way of saying that assumptions in modern mathematical economics finance theory are sort of like the old joke about the physicist being asked to explain an elephant and saying, "Imagine a spherical elephant." Well, the spherical elephant, you might say, is market efficiency. And some of the things that have happened financially in the last couple of years show that maybe someone should take a very hard look at that assumption.

ASPATURIAN: That's for sure. You asked me to remind you about Clair Patterson.

TOMBRELLO: Ah, Pat Patterson. He never used Clair. A genius. A very unusual genius. Lots of stories and I know some of them, and I knew Pat pretty well. There were times when I knew Pat extremely well. Pat had done something absolutely remarkable. He never won a Nobel Prize, but he won the Tyler Prize for Environmental Achievement [1995].

ASPATURIAN: Yes, he did. Not long before he died.

TOMBRELLO: The whole lead thing. The lead thing was an enigma, because you knew there was a lot of lead being dumped in the atmosphere because so much tetraethyl lead was being used as an anti-knock agent in gasoline. And yet every sample you looked at had the same amount of lead in it. It was a lot of lead, but it was the same everywhere, no matter how you measured it. There was one interpretation of that: The environment is full of lead and it's always been full of lead. Then along comes Patterson with *his* assumption, which was that everybody has been running contaminated samples for years and they haven't figured out a way to get rid of the contamination. And Patterson figured out a way to get rid of the contamination and measure what was really in samples. The ice cores from the South Pole appeared—I must have been a postdoc that year, and I remember, it was so striking. You could *see*, you could track human development through the amount of lead in the environment as you went deeper into the ice. You could see the little blip—the amount rising—when humans first started making bronze. You could see the big increase when the Romans started using it for piping and plates and all sorts of things. Cups, wine goblets, made out of lead. You start to wonder if you put an acidic liquid into lead, what would be the effect? Pat brought this idea forward—just how were the Romans affected by this? Was the decline of the Roman Empire partly due to lead poisoning? That was Pat. Pat always looked beyond.

So he saw the big problem. He was a character [Sam Beech] in a Saul Bellow book called *The Dean's December*. He never admitted he was that character, but it was a pretty good description. For reasons I never completely understood, Pat had refused to be a professor at Caltech. Then, while Barclay Kamb was chairman of Geological and Planetary Sciences [1972-83], he and I had a conversation. Of course, I always adored Barclay. I said, "You know, there's something wrong here. You really have to make Pat a professor." And we argued. It wasn't an argument; it was a classic discussion with Barclay. You went around it. You went around it again. You went around it in a different direction. Eventually at the end of it, he said, "You're right." So he offered a professorship to Patterson, and Patterson turned it down *again*! Then, later—this must have been the late 1980s—Wasserburg became chair [1987-89]. The truth of all this—you know, what is truth? I am sure Patterson was a pain for Wasserburg to deal with. He responded by being difficult with Pat. And Pat began to be very unhappy, because now he was not protected. He was a senior research associate; he did not have tenure. He was not a professor. Then Gerry, I guess, was pushed out, and Peter Goldreich [DuBridge Professor of Astrophysics and Planetary Physics, emeritus] came as an interim chairman, and one of the big things that Goldreich did was to offer a professorship to Patterson again, and this time Patterson knew he had better accept it, because it was survival. That was a wonderful thing that happened in that period: not only that it was offered but that, in this case, it was actually accepted. It made the last years of Pat's life more comfortable.

ASPATURIAN: And he was elected into the National Academy of Sciences [1987].

TOMBRELLO: That should have happened decades earlier, of course, because he'd done something really big.

ASPATURIAN: Yes, he did. He changed the world for the better.

TOMBRELLO: But he was a wild man. Adorable human being. He was a really *decent* person.

ASPATURIAN: It sounds like you were very fond of him personally.

TOMBRELLO: I really liked him. I liked a lot of the people over there very, very much.

ASPATURIAN: It's a good division. Who else over there comes to your mind, in geo and planetary?

TOMBRELLO: Well, I think hiring Goldreich was an absolutely brilliant thing. The story of his being hired [1966] is probably in the Archives somewhere. Let's see—John Bahcall was at Caltech then. He and I had come about the same time.

ASPATURIAN: This is the neutrino astrophysicist John Bahcall [d.2005]?

TOMBRELLO: Yes, and we were young professors, untenured, I think, when this came up. We heard about this guy Goldreich at Cornell who had solved the three halves problem for the orbit of Mercury, and we decided we could offer him a senior postdoc position, which he turned down and took an assistant professorship at UCLA. That was OK, for a while. Goldreich was a bit of a jock. More than a bit of a jock. One day he was waiting for a squash court. The person using it was staying overtime, and I guess Peter knocked on the door, the window, and said, you know, it's our turn. This guy was, I believe, a quarterback. He took this badly and tried to beat Peter to death with a squash racket. Peter's very strong, but this was a full-time, much larger jock, and the encounter was clearly somewhat one-sided. The university basically tried to whitewash that, and Peter came to Caltech. And the rest is history. Peter is—

ASPATURIAN: A brilliant man.

TOMBRELLO: A brilliant man. He is remarkable in— People tell me about the importance of citation indices and things like that. I say, "Well, there's Goldreich." He never published very many papers, but every one of them was a gem, an absolute gem. They opened up everything. He and I used to run together, be jocks together. Very competitive person. Very interesting person. As I say, in addition to the science, he did one big, wonderful thing, which was to make sure Patterson got a professorship.

ASPATURIAN: Someone else you mentioned to me was Ahmed Zewail.

TOMBRELLO: Zewail is one of the great Caltech success stories. He came here young, as a junior faculty member. He came and did all the development of the femtosecond spectroscopy. Maybe modeling himself a bit on Linus Pauling, he built a scientific empire that still does interesting things. I do not see that winning a Nobel Prize has in any way slowed the kind of science that Ahmed does. It's quite remarkable science. He's an interesting, interesting man. There's a story about him that connects to Physics 11. A few years ago, I asked a student in Physics 11 named Milo Lin to look into the question of putting a big diffractive-optics telescope—a Fresnel-grating-type telescope—into space. Let's say a 30-meter one for, you know, surveillance. You could look down with it, read people's license numbers, whatever, from space. What are the technical problems of doing that? Milo did a good job on that problem. But I had originally gotten the idea because there was a project like it at Livermore [Lawrence Livermore National Laboratory]. So the next summer I said, "Maybe you'd like to go up to Livermore." He did. And he worked in a group doing theoretical chemistry. It was basically molecular dynamics that was sort of time-dependent—how complex molecules move around. He came back and told me about it and how marvelous it was. He was by then starting his junior year. He said, "What should I do?" I said, "You should go over and talk to Ahmed Zewail, because what you're giving him is the ability to mathematically model the stuff he's going to try to do in this new time-dependent femtosecond spectroscopy." I said, "This is perfect." It worked. They published some papers together while Milo was an undergrad. He is still here as a grad student working in this field and is clearly going to be one of the powerful people in the field. And Ahmed has done a wonderful job of developing this guy. It's one of those cases where Caltech can utilize its unique advantage of being very good and very, very small. We all know one another. You can be a physics student and somebody can give you a shove in the direction of something in chemistry that you really ought to look at because what you're doing now really fits it.

ASPATURIAN: That's right.

TOMBRELLO: So, Ahmed is interesting in that though he still wins a lot of prizes, he is still clearly in the thick of a lot of activities. I think he's on this education commission [President's

Council of Advisors on Science and Technology] that President Obama has now. We got into a discussion about that: How important is the Finland result? Because Finland always finishes right near the top of any international list in the quality of high school, pre-college education. The question is: Is any of what they're doing applicable to the United States? Ahmed is very much involved in that. He is one of those people who's also an extremely good citizen in addition to being a superb scientist. The Nobel Prize couldn't have happened to a better character. I remember being interviewed by the BBC in 1999, when he won the prize. I said, "You know, he's not like a chemist. He's like a physicist. He builds his own stuff. He doesn't buy it." And I said, "You have to think of this place [Caltech] as Hogwarts Academy. This is our local magician, our Harry Potter." They loved it, because at that point it was still totally amazing that this young woman—this former welfare mom, J. K. Rowling—was going to be richer than the Queen from selling those books. That's another case where something good happened to the right person.

ASPATURIAN: Shall we talk about your take, over fifty years, on Caltech's presidents and provosts?

TOMBRELLO: OK. Why don't we start with my beginning, which was DuBridge. He was president at the time I came here as just a postdoc—I don't even know if DuBridge *had* a provost before Bob Bacher. But certainly by 1962, Bacher was provost. This was a case of two people who could work the two sides of the street, got on perfectly, and seemed to respect one another absolutely. They both had done big things during the war. They both were capable of running things. I believe this was a perfect case where Bacher was Mr. Inside, although he clearly had a presence in Washington, and DuBridge was Mr. Outside, raising money for the institution. He was doing things in Washington.

ASPATURIAN: Sounds like they were a dream team.

TOMBRELLO: They were a dream team in many, many ways. DuBridge had been in the job a long time. Then in the fall of 1968, he got the offer to be science advisor to Richard Nixon. Nixon's politics were *his* politics. I remember standing in front of the fireplace during a party at Tommy Lauritsen's house, talking to him about it. This was an interesting case, because

DuBridge interpreted the job offer, I thought, incorrectly, and being a brash young person, I told him I thought he was interpreting it incorrectly. He said, “This is the Nixon administration’s recognition of the things I’ve done in science.” I said, “Well, I think that’s true. But at the same time you’ve got to figure out if you could work with this president. Your success will depend on whether the president talks to you.” Because when presidents talk to their science advisors, wonderful things happen, and when they *don’t* talk to the science advisor, nothing happens. And in the case of DuBridge, Nixon didn’t in fact talk to him. There was a period, I think, when the country really didn’t have a science advisor, after DuBridge sort of gave up on it.

But then an interesting thing occurred. We had to replace DuBridge. There was a search committee—Bob Sharp ran it. Insiders have a hard time being promoted to president, just like outsiders have a hard time being made division chairs or provost. You know every bad thing that this presidential candidate has done. Some of them are not bad things, but they’ve made enemies. Anybody who has been or done anything in the administration of a school has had to make hard choices, and on one side of a hard choice is an unhappy person. You know where all those bodies are buried if the person comes from, say, within Caltech. If they come from outside Caltech, there are bodies at some other institution, but you don’t know about them, or not enough about them. So Bacher was not likely to be considered seriously.

ASPATURIAN: Was he interested in the job?

TOMBRELLO: Hard to say. Probably, but I can’t prove that. The committee went through a whole series of people. The only insider who had a chance was Bob Sharp himself, and I gather his wife was not at all interested in that. Bob would have been a very interesting president. They went through a list and got nowhere. Then one of the trustees—I don’t know which one—said, “There’s this young guy in his early forties, secretary of the air force, named Harold Brown. Give you a choice, you guys. You find somebody. We’ll look at them seriously. But you won’t find anybody. It’s Harold Brown.” OK. Christy got very much involved at that point and brought in a number of people. Presidents of other universities. Credible, credible scientists. Guy Stever [Guyford Stever, then president of Carnegie Mellon]. James Fletcher—the NASA administrator under Nixon. Both had been students here. William O. Baker, vice president for research at Bell Labs.

In those days, candidates for president were on public view. They came and gave talks to the faculty. I remember Harold Brown coming to talk to Kellogg. We were down in the tandem accelerator control room. And Willy [Fowler] proceeds to try to lecture this guy. Whereupon, Brown really took control of the meeting and told Willy the way it was going to be. Very impressive. Extremely impressive. This was during the Vietnam War. This was a secretary of the air force. He had been director of Livermore when he was in his thirties. In some sense, he was a protégé of Edward Teller. Oh, my! He was identified with the military, the government—and with Edward Teller! Even in those days, especially in those days, physics was very much influenced by the old Oppenheimer–Teller thing—which side are you on? Look, Brown had been too young to be involved in any of that. But this was Oppenheimer country, and he was clearly identified with Teller. He may not have won our hearts, but he won our votes. It was one of the last times presidential candidates came and talked and got questioned. Really questioned. So Brown was an interesting choice, and he came here.

DuBridge, I thought, had been a wonderful president. But he'd been in a long time, and toward the end of his term the bureaucracy had gotten Byzantine. It was not effective. There was too much of it. Harold Brown almost immediately, using the Sylmar earthquake [1971] and the damage to Throop Hall as a bit of an excuse, trimmed it and made the place tighter, better run, more efficient. Was he a visionary? No. If I had to grade the Caltech presidents I'd known—I did that once as an exercise—both DuBridge and Brown get an A. DuBridge because of vision. Clear accomplishment. Respect of the faculty. Respect of everybody, including the government. Brown was different. He was not the scientists' scientist. But he managed the place beautifully. He was easy to communicate with. DuBridge didn't get around. Brown did. I can remember once—I was still a junior faculty member, an associate professor without tenure—looking up from what I was doing and there standing in my office door—this was after Bacher had stepped down and Christy was provost—are Brown and Christy, who have pounced on me. They were pouncing on a lot of other people, I gather—just making these on-the-spot visits. Let's go see what so-and-so is doing and talk to them. Question them. It was very interesting. As I think I've said, Brown could tell you yes or no, and if it was no, forget it. If it was yes, the check was in the mail. He was a very interesting man. In some ways, he was *my* president at Caltech. Did he and Christy make mistakes? Everybody makes mistakes. I think one of the things that *was* a mistake—because they didn't think it out clearly—was the social sciences. They were going to build up the HSS Division in the social sciences. We had very good people in the humanities when I came here—Hallett Smith and people like that. They were very, very good. And the Huntington Library, of course, was such an asset. We got people in the humanities because of the Huntington. It's been a wonderful, special relationship. I hope it's preserved forever. But we started hiring people in social sciences, and even then—I always have opinions about things, sometimes wrong, maybe mostly wrong—but one of those opinions was, This is a ratchet. If you hire good people, they're going to leave, because in this area we're a farm team. You hire bad people, you're stuck with them.

ASPATURIAN: In the social sciences.

TOMBRELLO: There is something to that, in the social sciences. That you can do it, but you have to be absolutely ruthless. You hire *only* the very best people. You get turned down nine times out of ten, and you just keep going.

We learned later that Bacher did try to hire Stephanie's father and brother as a package deal, which says a lot about Bob Bacher. It was not amazing to go after Robert K. Merton. He was well known in the history of science. Well known in the sociology of science, the sociology of a lot of different things. Inventor of the self-fulfilling prophecy, unintended consequences. He even invented the focus group, though he didn't like to talk about that one. But the package deal was to hire Stephanie's little brother, who was still down in the tall grass. However, Bob must have noticed that he had been a major player, even as somebody very young, in the option-pricing-theory thing. Bob knew about it. This didn't come out of the HSS Division; it came out of Robert Bacher. He had figured this out and went out after those two. Didn't get them, but it showed a lot about Bob Bacher.

But even so, I don't think they [the HSS Division] thought through the fact that they could be left in a situation where the social sciences were a perpetual farm team, which continues to this day. It was hard to keep the good people, and you couldn't get rid of the people you had tenured who might not be as good. It makes it hard to go forward in social sciences here. I think it would probably surprise Bacher, if he were still alive, that there has been minimal progress there. That's my very opinionated view. I think what progress there is has come at the expense of the humanities, which are probably not as strong as they were when I came here.

ASPATURIAN: So Harold Brown was called to Washington [1977].

TOMBRELLO: I had predicted it. I tried to warn people that he was sitting out a Republican administration and that he would disappear—

ASPATURIAN: Really?

TOMBRELLO: It was out on the street, and why people here didn't react to it, I don't know. But he was gone, and they were left with Christy as acting president [1977-78]. Christy did an interesting job, but not—he was never quite president. First, he was acting. Second, he didn't have the personality to be president. He's a very solid person, but being president requires more charisma. The Mr. Outside part didn't exist for him, and Christy may not even have handled the Mr. Inside part as well as he could have.

ASPATURIAN: I was going to ask, since we're on him, how was he as provost, in your opinion?

TOMBRELLO: He was very good when Harold Brown was here. He and Harold were a good team, an excellent team. It was when Brown left that— He was not a failure, it was just that we marked time for about a year. And then we picked Goldberger.

ASPATURIAN: And how did that come about? He's a very different choice.

TOMBRELLO: Yes. You might say with presidents, we tend to oscillate between academic types and managerial types. So we had an academic type, we had a managerial type, then we had an academic type. Murph had only been head of the Physics Department at Princeton, which is not a big operation anyway. Murph was not a successful president. Many would say he was a failed president.

ASPATURIAN: Do you know how he happened to be chosen?

TOMBRELLO: He was put up by the high-energy physicists, I think. I don't know much about the committee. Gell-Mann certainly was pushing him. Fred Zachariasen was pushing him. Again, candidates came and gave talks. He came and gave a talk. I don't know what happened on the committee. But he ended up being *the* candidate. He was not a successful president. He was smart enough to be.

ASPATURIAN: Were you, as a physicist, pleased initially to have this physics individual—?

TOMBRELLO: I was worried.

ASPATURIAN: You were.

TOMBRELLO: I was worried about this one. There wasn't enough of a track record of having run anything. DuBridge had run something. Millikan could run anything. And Brown, of course, had run a bunch of things. You *knew* the track record. You knew how successful he'd been at it. The question with Brown was, was he enough of a scientist? The question with Murph was, was he serious? And the answer was, I don't think he was serious. He was a great amateur president. A gifted amateur is the way I'd characterize him. He had political skills that were unexpected and were good. He was bright, obviously; he's a very fine scientist. He had operated in Washington. He'd been a founder of JASON. So he did have Washington connections. I don't think he understood that this is a full-time job. I think he and Mildred [Mrs. Goldberger] were rude to people. The trustees did not like it. He had Christy for a while and then picked Jack [John D.] Roberts [Institute Professor of Chemistry, emeritus] to be the next provost. Roberts is a great man. He was not necessarily a great provost, but—it can be a hard role to define yourself in.

I tell this story about provosts. A friend of mine, John Deutch, was dean of science at MIT. He had been in Washington as undersecretary of energy. Rice was looking for a new president, and I had arranged for Rice to take a look at him. They ended up offering the job to Deutch. And MIT, to keep him, made him provost. I remember that not long afterward we were both at a Schlumberger party, and I said, "Boy, you really put your foot in it, John!" He laughs and says, "No, it's a promotion." I said, "No, it's not." The person who had recently been the president of MIT—Jerome Wiesner—was standing there, because he was on the Schlumberger board, and he starts laughing. I think he's figured out what's happening. I said, "John, this is not the job for you." He says, "Why not? It's a bigger job than being dean." I said, "No. You're dean at MIT, or you're division chair at Caltech. It's like being quarterback of a really good football team. You know, you go out there. The crowd cheers. You throw the long ball and score a touchdown. They carry you off the field on their shoulders. You know, hey, you get to do big things. You call the plays. The crowd loves you." He said, "But what's the provost?" I said, "Middle linebacker. You're there to keep the division chair, the dean, from scoring the touchdown. You know, you're protecting the resources of the institution." I said, "Nobody loves the middle linebacker." Wiesner is practically rolling on the floor, it is so funny. He says, "I told you, John. I told you." [Laughter]

In some sense, the reason you don't love provosts is that they end up being the backstop. They end up having to tell a lot of people no. That's why it's very hard for a provost to become president, and I'm *very* curious as to what will happen at the University of Southern California. You had an enormously popular, successful president, Steve Sample. He's a wonderful man.

ASPATURIAN: So I've heard.

TOMBRELLO: I got to know him a bit at Bohemian Grove. He's very interesting, very, very successful. His provost, Max Nikias, has now become president.

ASPATURIAN: Interesting.

TOMBRELLO: I've talked to people who were on the committee. Kevin Starr, the state historian. Very enthusiastic, said the committee did a very careful job, looked at *everybody*. It was not a foregone conclusion they were going to promote the provost. Still, Max has his work cut out for him. He's now got to be successful in some way that distinguishes him from Sample. Those are big shoes to put your feet in.

ASPATURIAN: So, moving back to Goldberger.

TOMBRELLO: Goldberger, yes. Students, I think, liked him. As I say, he had a popular touch. I think he was rude to people.

ASPATURIAN: Intentionally or unintentionally?

TOMBRELLO: I don't think they were intentionally rude. I just think—

ASPATURIAN: Just the way they were.

TOMBRELLO: Yes. A bit self-important perhaps. I don't know what it was. But I believe the trustees were not happy with Murph or with Mildred. Inside Caltech, and I think I'm quoting Christy, "This was not a successful presidency." Then he picked Robbie Vogt as provost—

ASPATURIAN: Yes, I was going to ask about that. How did he happen to pick Robbie as provost?

TOMBRELLO: I don't know. I don't know the inside part of that. Robbie had been PMA division chair. I'd had my difficulties with Robbie. From there, Robbie was kicked upstairs. There was a fight almost immediately. Murph got to see the dark side of Robbie, the fits and anger that was just always uncontrollable. Also, Robbie probably had more vision than Murph, and in many ways more charisma. Because when Robbie is "on," Robbie is really "on." So you had the two sides of Robbie, and he and Murph just did not get on. The trustees got dragged into the middle of it, and I think I talked the other day about when all of that came unglued [Session 4].

ASPATURIAN: Yes, I do have a question about that. This maybe shows a little of my naïveté about university politics. But when a president and a provost just no longer get along, doesn't the provost serve at the pleasure of the president? Or is it more complicated than that?

TOMBRELLO: In this case, when the trustees get dragged into it, and they don't like the president, then it becomes more complicated.

ASPATURIAN: I see. I see.

TOMBRELLO: I think we lost a number of trustees over this.

ASPATURIAN: You mean they bailed from the board?

TOMBRELLO: They thought it should have turned out the other way, I believe—Murph leaving and Robbie staying. That's what I'm told. We recovered well, as I said the other day, choosing Barclay and Linda Kamb, the royal couple. Camelot—Arthur and Guinevere! That was a brilliant choice.

ASPATURIAN: And you feel Barclay was a good provost. Want to talk about that a little bit?

TOMBRELLO: Well, you're never going to get a word out of me that's negative about Barclay Kamb or Linda Kamb. There are people who have a hard time communicating with Barclay, and we'll get into that when we get to Everhart, because that's coming pretty soon. In 1987, I left for Schlumberger. It was clear that Murph was in his last days. I think the deal was that they basically told Murph he was sixty-five and sixty-five was the limit, even though he hadn't finished his second term. He was going to finish his presidency in nine years and not ten. Murph was not happy. Robbie had accepted—because of my machinations and Ed Stone's—the directorship of LIGO, which worked out very, very well, certainly for Caltech, maybe not for Robbie in the long run. At the time, it seemed to be fine for Robbie.

I came back in 1989, and Everhart was here. I have heard several stories from the committee about how Everhart got chosen. He was sort of unlike any president I could have imagined at Caltech. I think Murph had some great strengths. Everhart had some great strengths, too. One of them was that he built a powerful and interesting Board of Trustees, which was something the other presidents had not done. I have to give him credit for that. I don't think he understood how to manage his vice presidents, and I think they ran free. I think some of them were out to get him.

ASPATURIAN: When you say his vice presidents, you mean—

TOMBRELLO: Morrisroe and some of the others. I don't think they worked for him. I think they worked for their own ends, and Everhart did not control them very well. I tried to talk to him about that, and then he got blindsided by the Robbie Vogt thing, as Robbie came unglued.

ASPATURIAN: The LIGO business, you mean.

TOMBRELLO: Yes. But I think the trustees liked and trusted Tom Everhart, and they should have. He's solid. He's dependable. I think they liked that. And he worked very hard to build a very powerful Board of Trustees.

ASPATURIAN: When you say "powerful," you mean really supportive of and invested in Caltech?

TOMBRELLO: Supportive of. They played on a big stage. They were rich. They were generous. I would say he coupled the trustees more closely to Caltech than other presidents had, and they were quite a bunch. I don't think he would be— I would consider him a gentlemanly C as a president. Nothing disastrous, except the fact that some of his vice presidents were out of control. Morrisroe, who had done a fantastic job for Brown, was clearly running his own show. And that happens when someone who is one layer down in the organization is not integrated with the overall strategy. There probably wasn't an overall strategy—except the big strategy of getting the trustees organized. But Everhart was allowed to finish his term.

ASPATURIAN: I'd like to ask about the provosts under Everhart. He started with Barclay Kamb, is that right?

TOMBRELLO: I'm glad you brought that back up. Because I don't think they could communicate. It wasn't entirely Everhart's fault. Barclay communicates differently than other people. If you're a fan of Barclay, you take the time, because in that communication is wisdom. As I mentioned when we talked about my being at Schlumberger, Barclay, Frank Press, John Deutch, and I were on the original Schlumberger visiting committee. Even the people on the committee, including Press—who was a *fan* of Barclay's, because Press of course had been here and run the Seismo Lab before he went to MIT—were frustrated with Barclay. I thought Barclay was wonderful. I still do. He's one of my heroes. If it's hard to communicate with Barclay, take more time. It's worth it. But I don't think Everhart could do that, and he got more frustrated with it—and, as I say, it got kind of out of control, because I believe there were people trying to push their own agendas. Barclay was kind of in the way. I think a lot of people were campaigning to be provost.

ASPATURIAN: I have a question. The story I've heard, and I don't know if it's accurate, was that Barclay had something to do with Lee [Leroy E.] Hood's dismissal from the Division of Biology [division chairman 1980-89] and that this, in turn, caused problems that led to Professor Kamb's stepping down from the provost's position.

TOMBRELLO: That I don't know. Lee Hood was a friend. I still see him occasionally.

ASPATURIAN: He's in Seattle [Institute for Systems Biology] still, I believe.

TOMBRELLO: Yup. Doing his own things. Lee I had first met because he was a friend of Andy Bacher's when he was a grad student. He'd come back from medical school to be a PhD student in biology. He's a very interesting guy. Knew them, knew Mary Ann. Mary Ann? No—

ASPATURIAN: Valerie.

TOMBRELLO: Valerie. The kids were Mary Ann and Leo. They were about the same age as Kerstin, our youngest. I have always been a fan of Lee Hood. But he played by a different set of rules. Was somewhat bigger than life. The Biology Division is not sociologically wholesome. Maybe one of their problems. They're good, but they don't work together—they don't play nicely with others. Lee made enemies within the division and that probably ended up getting him, as much as anything. I believe the provost gets caught in the job of solving that, sort of like the Wasserburg thing in GPS. You have to get rid of them and put somebody else in, and in the GPS case they put in, for a short time, Goldreich. Goldreich was willing to do it as an interim division chair.

ASPATURIAN: So after Barclay Kamb, we had—was it Paul Jennings [professor of civil engineering and applied mechanics, emeritus]?

TOMBRELLO: Yes, I believe that's when Jennings came in [1989-95]. Paul was an interesting choice, and clearly, in many ways, the first coming of Jennings was maybe not as successful as the second coming. [Jennings served for a second time as provost in 2004-07]—*ed.*] But at the same time, it was successful. He got caught up in the LIGO mess. And we were slightly on opposite sides. But he would listen to me. I said, "You know, you have to listen to Drever, because there is a story there—"

ASPATURIAN: That would be Ron Drever.

TOMBRELLO: Robbie just got frustrated with Drever and found that, you know, he just couldn't deal with him. The institute really didn't know how to handle it. They wanted to support Robbie. They brought in Lew Allen, who'd stepped down—finished his term—as head of JPL, to head up the LIGO committee. Interesting man. I can remember when it all came unwound. We'd gone to Washington, middle of a snowstorm, ice storm. Someone whose name has now slipped my mind, but he was there as kind of an advisor, asked me if I was thinking of running. I said, "Oh, no. I think Barry Barish is the obvious choice. The SSC [Superconducting Super Collider] has just been shot down. He's clearly capable of it. Technically sound. He doesn't have to prove that he can run it. He can run it." We just sort of decided that while looking at our plane being unstuck from the ice in Washington. They had found this LIGO advisory committee. I got put on it. A story of that was interesting. That must have happened in the fall of '92, maybe.

ASPATURIAN: I'm trying to remember what you said in your LIGO oral history. That sounds about right.

TOMBRELLO: And I may have told this story. Stephanie is back East, because her mother has cancer and probably does not have long to live. I can't remember the exact time scale, but it was short. I was here, and I went to the president's Christmas party. I was met at the door by Lew Allen, *grinning* at me—Lew didn't grin in general—and he said, "We have plans for you." I said, "Oh, my." That's when we decided this LIGO oversight committee was clearly an important thing to have. That was also an interesting story, because, after I got away from him, I went in the kitchen to get a drink and there was Christy. I had just read a book and I can't remember the exact title, but it was something like how Stalin got the bomb. Because a lot of files had gotten opened up right after the Soviet Union came apart.

ASPATURIAN: Yes, that's right.

TOMBRELLO: I said, "I know what the Russians tested, that first mysterious test." Then I just grabbed my drink and disappeared. Christy followed me like a bloodhound. Found me and cornered me in one of the rooms and said, "All right. What did they test?" I said, "They tested Teller's Clock." He says, "Oh, my God! That's what it was." Because it was a mystery test. It didn't look like the thermonuclear weapons that the U.S. had just tested. It looked different, but it was obviously thermonuclear. Teller had this design. It was as big as a whale, and it would never get more than 400 kilotons. It was the only part of his research sort of publicly out—people at Los Alamos knew about it and Fuchs was still around.

ASPATURIAN: Klaus Fuchs, the British spy.

TOMBRELLO: And the U.S. wouldn't build it, because, as Oppenheimer always said, "Inelegant. It doesn't scale. It's as big as a railroad car." But the Russians built it and tested it. And it was Teller's Clock. It was just an eye-opener to Christy. "Of course that's what it was!" I said, "Yup, that's what it was. It was the Clock." The "clock" comes from the way that Teller had described it—an alarm clock to wake up Joe Stalin. The classical Super was something different. The story of that is all kind of mixed up. How did the Russians get it? Was Sakharov [Andrei Sakharov, father of the Soviet hydrogen bomb] really that smart that he duplicated it? Even though they claim that they didn't analyze fallout because they inadvertently threw the samples away, I still think that the Russians figured out how the Super worked by analyzing the fallout from the U.S. test. Because once you see that—if you've got really smart people like Sakharov—you can back-engineer it. But the Russians claimed no. So that was an interesting Christmas party at the president's house.

Then we put together this LIGO advisory committee. Then we ended up in Washington, having to replace Robbie and that's how Barish got picked. It was all a bit of serendipity, because the SSC had been canceled. This big detector that Barish was responsible for was not going to get built, because there wasn't going to be an accelerator, and Barish was available. The rest is history.

I'll have to tell the story about Robbie, about Barclay's firing. That occurred probably just after I had come back from Schlumberger, in the spring of '89. Barclay had been fired. He and Linda threw a party for the people that had basically expressed their dismay.

ASPATURIAN: Now, was he let go by Everhart or was it by the trustees?

TOMBRELLO: Everhart. There were a lot of people campaigning for the job, but it ended up, I think after maybe one false start, with Jennings, who was a good choice. Jennings is very sound, very smart. He could work things through.

I adore Paul Jennings. I'm going to jump ahead and talk a little about how clever Jennings was and how sound he was. I was PMA division chair, and I had gotten a number of endowed chairs for people in the division.

[PORTION TEMPORARILY CLOSED, pages 143-144.]

ASPATURIAN: Before we return to Paul Jennings's second tour as provost, I'd like to get to where your onetime protégé becomes the provost. That will keep it chronological, to some degree.

TOMBRELLO: Oh, yes! Well, I had mentioned earlier that when [Gerry] Neugebauer was finishing his five years as PMA chairman [1988-93], he clearly was very unhappy with the administration. So they had a search committee, and I mentioned earlier that [Roger] Blandford had run the committee and had waffled and ended up sending four names forward, and Charlie Peck got chosen. Koonin was not chosen, and he was quite unhappy about that. But the next job to open up was provost, and he got that.

ASPATURIAN: And he'd been chair of the faculty, I believe.

TOMBRELLO: He had been chair of the faculty earlier.

ASPATURIAN: What were the circumstances of Koonin's being appointed?

TOMBRELLO: I don't know, precisely; I never got any insight into the committee. I didn't think it was an unreasonable choice. When he got in, there were some rough edges to start. He'd never had an administrative post at Caltech. He got crosswise with a number of people, including Ahmed Zewail and some others.

ASPATURIAN: Really?

TOMBRELLO: Koonin had opinions, and he didn't mind telling you what they were. Remember, he was a teenage protégé of mine, and maybe he got some of the wrong lessons. One was to be a bit of a dilettante in science and do a lot of different things. And the other is sometimes to tell people things they don't want to hear.

ASPATURIAN: Are you comfortable giving a couple of examples?

TOMBRELLO: See, I've never had any trouble with him, but he clearly had not treated Zewail with quite the respect that Ahmed wanted. Again, Koonin had not done any administrative stuff, and he was thrown into the provost's job. He made some mistakes by offending people, basically by shooting from the hip on a few things. I was willing to excuse it. After all, he'd been my student. I'd gotten him *back* here. I had kept him from getting away; I told that story. To jump ahead just a little bit, when I had an interview with David Baltimore about becoming division chair of PMA, he said, "Well, Koonin was your undergraduate student and then your colleague. How do you feel about working for him?" I said, "Would 'proud' be a good adjective?" That finished the conversation. I said, "I have no problems." Koonin and I have had our disagreements. Haven't we all? I said, "But I think, you know, I think I can work with him. Will it always be smooth? Why should it always be smooth?"

Turned out it was exceedingly smooth. I'll give an example: He was chosen provost in 1995, and I got chosen as division chair in '98. I got in just before the beginning of a new fiscal year—I believe, the first of August in 1998. I discovered that the budget for the next year had been completely committed. I had discretionary money, as division chair of a big division, of $19,000. My predecessor, Charlie Peck, had committed it all, and I wasn't entirely happy with the way it had been committed. So I cried foul to Koonin. [See also Session 8] Koonin looks at me with a twinkle in his eye and says, "You're really good with money, Tom. Let's just say this is a challenge for you." I said, "Well, Steve, I'm willing to take that challenge." I said, "I don't think anybody should look at the books *very* carefully. And with the current financial system, you really can't. It won't be illegal, but I'll get through the year with $19,000 of discretionary money." He just laughed. To make a long story short, I finished the year with a surplus, small. I got a lot of things done. I renormalized expectations in the division. The next year Koonin silently applauded and gave me a better budget. But I had to prove it. I had to prove I could do it. There wouldn't be any outcry. There wouldn't be any *obvious* financial irregularities. You have to put an underline under obvious. It worked. So working with him was a lot of fun. Never perfect, but very, very good, because we'd known one another a long time. We knew where we were coming from. It worked very, very, very, very well. And worked to the benefit of the division. We started the Thirty Meter Telescope project, the two of us, and brought the astronomers in.

ASPATURIAN: I'm going to ask you to hold on that until we get to your years as PMA chair. But at the time Koonin became provost, you were still dealing with, was it, the staffing committee for physics?

TOMBRELLO: Oh, yes, I had the staffing committee, except for that break at Schlumberger, since 1986.

ASPATURIAN: So that must have been—

TOMBRELLO: That caused some interesting problems. Because I don't believe that Koonin had a huge amount of respect for Charlie Peck. Not as much as he should have, actually. Part of that was, of course, that Peck had been chosen over Koonin. Peck had been chosen over me, too. But, you know, I figured, Hey, I can work with this! I'm still running the staffing committee, and Charlie isn't getting in my way. Charlie did one thing that I had to be myself about. I'd been teaching Physics 11. That was the deal. Charlie then, basically, tells Frautschi to tell Tombrello—

ASPATURIAN: [Professor of theoretical physics] Steve Frautschi—

TOMBRELLO: Yes. He tells Frautschi, the executive officer [for physics], "Go tell Tombrello he's going to teach Physics 1 for a term." I tell Frautschi, "No!"

ASPATURIAN: That's the introductory course?

TOMBRELLO: Yes. I'd taught it many times in the past. But I had a deal with Caltech. I was teaching Physics 11. I had won the Feynman prize, I wasn't about to teach Physics 1. Everhart says, "Who do you think you are?" I said, "I know perfectly well who I am. I'll teach it for a thousand dollars a lecture. It'll be a bargain. It'll be wonderful. But I'm not putting up with this crap." And Charlie Peck came in about something, and I basically chewed him out for about an hour. I ended up not teaching Physics 1.

But Charlie and I got on pretty well. We'd known one another since I came here in '61. He'd been a grad student who was just about to get or had just gotten his PhD [1964]. I know Charlie and respect him. He's good. He's smart. He was not a great division chair, but he had problems that, you know, were not easy to solve, and he solved some of them and didn't solve others. You could say the same thing for me. Same thing for anybody who gets a new job like that.

So, the staffing committee. There was a situation where the committee found two candidates—one in string theory and one closely allied. But they were theorists, and we needed to start rebuilding theory.

ASPATURIAN: Who were they?

TOMBRELLO: [Juan] Maldacena, who's at the Institute for Advanced Study, and a guy named [Kenneth] Intriligator, who is, I think, still a professor down at UC San Diego.

ASPATURIAN: Is he any relation to Mike Intriligator?

TOMBRELLO: I don't know who that is, but I believe his mother is a planetary scientist at UCLA.

ASPATURIAN: I think he is. I know Mike [Michael D.] Intriligator. He is a UCLA economist. This must be his son.

TOMBRELLO: Koonin had basically told Peck he'd accept one candidate. The committee—remember now, I had built this committee into a powerhouse, because it had all these opinionated people on it; I think we even had Robbie on it—basically put our foot down and told the division chair we wanted these two candidates. Go back to the provost and say the committee *wants* it.

ASPATURIAN: The provost at this point being Koonin.

TOMBRELLO: And Koonin really treated Peck rather badly. With not much respect, and I think everybody deserves respect. But after all, this was Charlie's job. It was our job to put forward a vision for staffing, and it was Charlie's job to try to sell it. And if the upstart provost wants to take exception, let him. [Laughter] But it really hurt Charlie Peck's feelings, the way it was handled. He came close to, I think, resigning over that.

ASPATURIAN: But did either of these guys come?

TOMBRELLO: No, but we sure tried. Maldacena has turned into one of the real hot shots in the field.

But Koonin and I were critical fans of one another. We had been through the fire together before. We knew how far we could push one another, and we knew what we could expect of one another.

ASPATURIAN: Yes. Sometimes that makes for the best synergy, doesn't it?

TOMBRELLO: It made wonderful synergy. I enjoyed working with Koonin. I enjoyed working with Jennings. I thought they were superb provosts. And very different!

ASPATURIAN: What do you consider Koonin's major accomplishments as provost? This takes us into the Baltimore era, but we'll get to Baltimore in a minute.

TOMBRELLO: Well, Steve did a number of things that are sort of at the margin, but which I think improved things. He got the tuition aid program for the faculty. That was, I thought, important; it was an idea whose time had come. Clearly, we had to have something. He was the one who started kicking back money for faculty salaries that were paid on grants. He didn't kick back the overhead or fringe benefits, but he kicked back some fraction of the salary. That was important.

I believe he had a vision for various parts of the fields. He had a vision for biology and a vision for quantum computation, both of which, I think, were successful. He had a vision with me for the telescopes of astronomy. He had started, and I helped him carry through successfully, the ASCI Program [now known as ASC, the Advanced Simulation and Computing Program—*ed*.].

We're going to skip ahead for a minute, and then we'll pick this up when we talk about Livermore and my consulting on weapons [Session 7]. Back in the middle-to-late nineties, we realized we were never going to test nuclear weapons again. Some of us picked this up much earlier than others. I remember having to tell George Miller—the guy who is now the director of Livermore, when he was just further down the pack—that he was never going to test a weapon again. I said, "It's not something I completely agree with, either, George. But, it ain't going to happen."

ASPATURIAN: So the question became, What are you going to do now?

TOMBRELLO: What are you going to do now? That's when Vic [Victor H.] Reis in Washington came up with the idea of a stockpile stewardship. How much could we turn this into an engineering problem of predicting the way weapons aged in the stockpile? They threw open the idea of this ASCI program, which was kind of thinly disguised weapons research but had nothing classified in it. So you could have your postdocs from China working on it. They threw it out to the universities, and Koonin made sure we got a piece of that. I helped Koonin structure the part of the project for Caltech, because I knew how the damn things worked, and I knew where the boundaries of classification were and also where some of the potential problems were. But the way I set it up, there was nothing you couldn't just talk about in meetings and have your foreign grad students work on, and this was tricky. It was not entirely popular with people like Roger Blandford, who wanted to work on something that was, let's say, borderline fusion, because it fit beautifully in with the astrophysics.

ASPATURIAN: He was still here at that time?

TOMBRELLO: Yes. I felt that left room for too much mischief. I wanted to keep it clear of what I consider proprietary technology, thermonuclear weapons technology. I wanted to keep it sort of at the fission-trigger level. I kept the geometries away from spherical and made sure they were more cylindrical, which has nothing to do with bombs. But they still contained the ingredients that would give us some new mathematical tools for modeling. I think Dan [Daniel I.] Meiron [Jones Professor of Aeronautics and Applied and Computational Mathematics] had the strength of ten. He was wonderful. Did a good job. Interestingly enough, this whole project dragged people like Dan Meiron and Mike Ortiz [Hayman Professor of Aeronautics and Mechanical Engineering] into the weapons community as really trusted, knowledgeable advisors on things that had to do with weapons. I think that was important, a breath of fresh air—a number of people who just thought about things different ways and were just smarter than hell.

ASPATURIAN: You worked with Koonin to bring this off? And you worked together well on that?

TOMBRELLO: Yes. Very, very well. He never doubted the boundaries I drew. Steve's experience with weapons was mostly through the JASON group. I consider that—to be opinionated—a bit superficial. That says nothing about Koonin; it says more about the JASONs**.** They looked at things from the top of the mountain, and there I was, where the plutonium met the whatever.

ASPATURIAN: Where the plutonium met ground zero?

TOMBRELLO: Something. My perspective was always at the level of the details. It was not a view from above. It was not about policy. It was strictly, Will X do Y? And what happens when X gets thirty years old? Will it still do Y? So it was a very successful program for Caltech. It ran for a number of years.

ASPATURIAN: And obviously you consider it a key contribution.

TOMBRELLO: I thought it was a key thing Koonin did for the nation. I think it did a lot for various groups at Caltech, including providing a view of how some very important things in U.S. weapons policy are dependent on having smart people comment and know something about the technologies underneath it. It is *not* all just an engineering problem. Engineering solutions are fine. We got a long way with that. I do not denigrate them. But, at the same time, if you've got to predict something that may be outside the range of what you've tested, then science is the only thing you've got. So it was this integration of science and engineering and modeling—a troika, if you like—that was important. I think Koonin—I mean, I'm clearly taking credit for part of it—but Koonin was the driving force. He got it here. He had the vision that we could put this thing together. I think I was just a handmaiden to it.

Same way with the TMT [Thirty Meter Telescope] project. We were on the same page. We trusted one another. We had enough history that we could forget about any disagreements we had in the past and get on with it, and not get upset when the other person didn't agree with the next step. We could talk it out.

ASPATURIAN: I'm getting a very clear picture. Yes.

TOMBRELLO: I would like to think in a minor way, it was what Bacher and DuBridge had done. And if you will notice, while I was division chair, I've always had this photograph where I could see it. It was Robert Bacher, sitting in the chairman's office in physics.

ASPATURIAN: [Examining picture] The bust of— Is that Newton back there?

TOMBRELLO: Let's see. Probably Ben Franklin.

ASPATURIAN: Ben Franklin. Similar wig.

TOMBRELLO: So Koonin said, "Why do you have that up there?" I said, "Because any day I figure I'm really doing it well, I look over and derive a certain amount of humility from the fact that that guy [Bacher] did it a lot better. And I wonder if he would think I was doing well." I said, "It's a great normalizer to be basically standing on the shoulders of a real giant."

ASPATURIAN: So, we pass from Everhart to Baltimore, with Koonin still as provost. What is the history, to the extent that you know, of Baltimore's selection as president? This was just before you became division chair, I believe.

[PORTION TEMPORARILY CLOSED, pages 152-173]

THOMAS A. TOMBRELLO

SESSION 7

December 28, 2010

ASPATURIAN: This is December 28th, and we are going to talk about your involvement in activities at Los Alamos and Lawrence Livermore. National laboratories.

TOMBRELLO: As a student at Rice, I'd always been intrigued by Los Alamos because of what had happened there during the war, and because I knew some of the people. A curious thing happened when I was an NSF postdoc for the first time here at Caltech. I figured the postdoc wasn't going to last more than a year, and while I was looking around for what to do next, some interviewers from Los Alamos came to the campus. I interviewed and very quickly got a letter back saying they weren't interested. OK, I figured, that's par for the course. Then I went off to an American Physical Society meeting in Washington and ran into somebody named Lawrence Cranberg, whom I had never met, but I knew his work—and he was at Los Alamos. He saw my nametag. Pounced on me and said, "I am so excited that you are interested in coming to Los Alamos. I'm really looking forward to having you there." And I said, "Larry, you've got to be joking." He says, "No. I'm not joking. I'm very enthusiastic about this." I said, "Well, several weeks ago, I got a rejection letter from Los Alamos." He says, "Several weeks ago? I have only had your CV for a couple of days." So that was my first connection to Los Alamos. [Laughter] I was fired and then hired, but by then I had already agreed to go to Yale. I don't know what would have happened if I'd gotten a more positive response; I probably would have gone to Los Alamos.

One of the things that happened right after we came back to Caltech from Yale was that I got a call from Los Alamos again. We'd driven across the country twice in one year in the middle of the winter, so we were a little shell-shocked. I was offered the possibility of becoming director of the cyclotron there. Remember, I'm still in my twenties; I'm a postdoc again, and had been an assistant professor for maybe a total of six months or something. So I just couldn't move my family again, although it was an attractive offer. I was at Caltech; I was having fun.

And then in the summer of 1971—so now we've moved ten years forward—I'd been doing some work on accelerator design. The people at Los Alamos got interested in some of those designs and invited me to come. At the same time, I was on a time-allocation committee for an accelerator that was just being built there called LAMPF [Los Alamos Meson Physics Facility], which was a high-energy nuclear physics facility, with a big linear accelerator. The whole family went, and it was a wonderful experience that was repeated for the next two summers, most of which I spent on this accelerator design project. It didn't go anywhere, but it was a lot of fun, and I published some papers on it. And of course, the scheduling committee at LAMPF continued, so I continued going back and forth to Los Alamos after that. It all worked well until the early eighties, when Jay [George] Keyworth, who was head of the physics division at Los Alamos, went off to be Reagan's science advisor. I think that they wanted to burn all trace of Jay and anybody who had had anything to do with him. Suddenly, I discovered I was no longer welcome at Los Alamos. They canceled my security clearance—not that I'd been doing very much that had anything to do with national security at that point. But a friend of mine, Tom Sugihara, who had been dean of science at Texas A & M and had gone on to become a kind of a guru to the head of the chemistry directorate at Livermore, realized suddenly that I was up for grabs. He got me involved with Livermore. I'd been up there; I'd given talks. I knew people there, but I'd never really had any relationship with them.

What he wanted me to do was some organizing. They had two new visiting committees there, but they were rather haphazard and really didn't do much. Tom said, "I'd like you to come here. Organize a committee—two committees; one for the materials science half of this directorate and the other half for the chemistry side. I want them to look like the reviews of Argonne National Laboratory."

At the time, actually, I was on the Argonne review committees, so I knew about how it worked. At Argonne, you had a number of diversely organized committees, reporting to the University of Chicago, but at Livermore we were reporting to the lab's upper management and the University of California. I agreed to study it and to set up the two committees. All this began to really *happen* about the time I went to Schlumberger. So I was doing it while I was at Schlumberger, too.

ASPATURIAN: Two management portfolios at once.

TOMBRELLO: Yes, it was an interesting challenge. But it was different. At first the committees did not have to be completely cleared for security. The lab declassified the stuff we were looking at. We were looking at the plutonium facility. We were looking at the tritium facility. We were looking at a bunch of things and setting up a committee to try to comment mostly on science—the quality of the science; not its applications. It was as if you've got an iceberg and you're only looking at the part above water, which is about 10 percent. The committees were slightly frustrated by it.

ASPATURIAN: Was Livermore at that time still mostly in the weapons business?

TOMBRELLO: They have always mostly been in the national security business. But there is a veneer of science. There is an underpinning of science as it applies to the weapons stuff, but some of the frustration of those first committees had to do with the fact that you were seeing only the tip of the iceberg, even though you knew there had to be some reason they were doing the science. Those of us with Q clearances—we knew, and we could find out. But some fraction of the committee wasn't cleared, and so they weren't informed. But OK. We did this. We began to write systematic reports. In the process of all of this [in 1988], Roger Batzel had stepped down as director and John Nuckolls had become director. Nuckolls—to put it in perspective—had been head of the laser division, and almost the day after the invention of the laser was announced, he had basically proposed that you could potentially use lasers to implode a fusion capsule and make energy. Laser-driven inertial fusion. To show that's still around, I'm on a National Academy [of Sciences] committee right now, which has just met once—I met for sixteen hours on the telephone from Kauai, rather than fly to Washington in a blizzard—to look into what we are doing to push this forward for energy.

ASPATURIAN: Laser-induced fusion.

TOMBRELLO: Yes. And the person who is paying for this is the undersecretary of energy for science, a former undergrad of mine named Steven Elliot Koonin. He put me on the committee, I think, with malice, saying, "Well, we have to have somebody who's a fan but who is a very critical fan." As you noticed in these interviews, I can be very critical. I can also be a fan, and I think you need that combination. So I'll be interested to see how this develops. I believe the reason for this is that Koonin thinks there's going to be a breakthrough in this.

ASPATURIAN: Does he really?

TOMBRELLO: And that if it happens, somebody is going to reasonably ask the Department of Energy, What are you doing next?

ASPATURIAN: Because you know the old joke about plasma fusion—

TOMBRELLO: It's a great future source of energy that will always be in our future. [Laughter] Oh, yes, indeed, and that joke went around the committee the other day, which was barely a week and a half ago. If you've had your ears stuck to a cell phone for sixteen hours over two days, you remember it. But it would have been worse, going to Washington and getting stuck in a blizzard. They were all very envious of me describing the palm trees and the fact that I was drinking a glass of wine.

So I got these committees started, and they were successful almost immediately. The lab management liked it. The University of California liked it. I believe we provided a conduit for people working there both to find out what other people were doing and to get their views known to the higher-ups.

ASPATURIAN: Now, this was the late 1980s?

TOMBRELLO: Right. I think the first committee meeting was probably in 1988. That was the chemistry committee, followed by the materials science committee. OK. I go back to Caltech, and this continues. We had one memorable meeting when I was flying up on Southwest Airlines. I had decided that for variety I would fly into San Jose instead of Oakland. October 1989. The pilot announces there has been a big earthquake and the Bay Bridge has been damaged, and I thought, "It's not April Fools' Day. What is this?" But we land without incident at San Jose. It was the only open airport in the whole area. Some of my committee never got there. They circled and never landed anywhere. But I figured out a way to get to Livermore without crossing anything that was trouble, although I realized that the streets perpendicular to the ones I was on were totally blocked. It was an interesting experience.

But the committees moved along, and I'm going to telescope it all, because, you know, the details don't matter too much. The lab continued to evolve, and I kept evolving the committee. The only thing that didn't change was that I continued to chair both of them. Associate directors that ran that directorate changed, and the nature of the committees changed. At one point, the chemistry committee had to pick up nuclear chemistry, and you couldn't really do nuclear chemistry unless you dealt with the classified issues. The nuclear chemistry group basically was the group that did what were called secondary diagnostics. I guess it's no big secret that a thermonuclear weapon is a primary fission weapon that then detonates the secondary, which is the fusion weapon. I won't go any further, because you can find all kinds of crap on the Web, and I can't even comment on the accuracy of the stuff on the Web about it. But the secondary diagnostics mean you're looking at radioactive materials from both the bomb itself and from little markers you put in there to tell you what's actually going on in this explosion. A subset of the committee had to be cleared, and we separately wrote a report about that, which then went through the declassification process and was incorporated into the main report. So that was the beginning of the fact that the committee was going to deal with classified material. As things continued, it got more and more important that the other 90 percent of the iceberg get looked at. The committees had to be completely cleared for security. Eventually, it became slightly embarrassing that I had been doing this for not quite twenty years, and so I stepped off the stage.

ASPATURIAN: What year would this have been?

TOMBRELLO: 2006, 2007, somewhere in there. I still was on some other committees and got pulled in on a lot of ad-hoc committees. Ad-hoc committees plus red teams. Red teams are an interesting exercise. I've done that for JPL, and I've done it for Livermore. It's usually when you want to find out why something went wrong that you form these committees, and I believe JPL calls them tiger teams. At Livermore they're red teams—no political connotation. You're supposed to meet as an ad-hoc committee, come to a conclusion, and write a report, which is handed to somebody farther up the line. In the summer of 2008, I was chair of a red team on nuclear counterterrorism. It was a very good committee, including a former director of Livermore named Bruce Tarter.

ASPATURIAN: Was this a Livermore-based team?

TOMBRELLO: The people were mostly from outside. There were two people basically from the lab. One was Tarter, who had retired as director some years before. And one was General John Gordon, who had been associated with the University of California and was director of Homeland Security for a while. It was interesting.

ASPATURIAN: Was this committee under the umbrella of Homeland Security?

TOMBRELLO: No. It was under the umbrella of the lab.

ASPATURIAN: OK. So they were the initiating agency.

TOMBRELLO: And I chaired it. The goal of the committee as I enunciated it—because it was my idea—was not just to write a report but to write a report that would end up on the desk of the new president. We didn't know who it was going to be. It could be Obama. It could have been McCain. But we felt that this was important. Our analysis of it, we started off jokingly, would be based on this discussion of the 3:00 a.m. phone call. If you remember, that was the Hillary Clinton campaign ad that asked, Whom do *you* want in the White House when the emergency call comes? So we asked, What would be the substance of that 3:00 a.m. phone call? What would be something so urgent and critical that you would wake up the president for it? We decided that one of the highest probabilities was that some foreign country or terrorist group, particularly a terrorist group, had a nuclear weapon or had the materials from which you could construct one. And probably that *truly would* be a 3:00 a.m. phone call. We wanted to analyze this problem from the point of view of how well would the information that we knew about, and were getting presentations, travel through the system, and how good would be the information on which the president would have to make a decision? We wanted the new president to see this.

The upshot—to make a long story short—is that Obama has spoken several times in public about the proliferation issue, the nuclear counterterrorism issue, the stockpile issues; and it is very interesting to hear words that you wrote, or that you caused to be written, coming out of the president's mouth. It's a good feeling, tempered by the fact that one of the findings of this report was that a lot of the pieces are good, will work—and the system will fail at nearly every boundary. Because of the timing, we used the word "Katrina" many, many times. We said that no matter how good the pieces are, if they don't work together and the information doesn't cross boundaries, the president will have nothing to make a judgment on. Many of the recommendations are totally classified, but the upshot was, You people have to figure out how to work together. You're going to have to do a lot better than Katrina. I know it was a low blow to keep mentioning Katrina in there, but the director of Homeland Security didn't have any objection to it.

John Gordon is a very interesting man. I enjoyed working with him. I didn't expect much. I've known a bunch of four stars in my varied career, and they may be good at military but they can't write. They think in PowerPoint and that kind of thing. But not John Gordon. He wrote at least as well as I did, if not better. He edited extraordinarily well. Livermore got spun off from UC because of reasons that don't make a lot of sense, but it's now run by Bechtel and the University of California, and so they have a board and John Gordon sits on that. I was lucky enough to get him on this red team. But that gives you an example of the kind of thing a red team might do.

Then I got pulled back onto the committee that I had started. I'm not the chair. A friend of mine is. In some ways this is a lot better. You're responsible for writing a lot less and you're less responsible for pulling the whole report together. I had a philosophy, which they loved. So many committees will meet, take notes, listen to testimony and presentations, and then the report takes forever to get out. I demanded, successfully, that a draft of the report be written before the end of the meeting. So when you have your final debriefing with the laboratory management, you're going to tell them exactly what you found, and they're going to hear it in real time. Because one thing I've learned is that a report that comes out more than about a month after the fact is dead on arrival. This way, they may get their final report a week or two weeks after the last meeting, but they have heard it all at the last meeting. It's all been pulled together. It's all been integrated to the extent that you can. It's a snapshot in time. Nobody expects these reports to be anything more than a very good snapshot by people who are good at getting details right then, and therefore it has to be immediate. I'm a great believer in this. The lab got terribly spoiled by that. The new chair of the committee has bought into this completely, which is, "Gentlemen and ladies, we're going to write this thing right now. When we talk to them, these are going to be the conclusions and they're not going to change between now and the final report, which is going to be a week or two weeks from now."

I used that approach also on the Argonne committee. I remember one time I was not chairing the Argonne committee, and the report came a year late. It came about the time of the next meeting of the committee. It was of absolutely no use whatsoever. I like committees that are tightly run and controlled, with a well-defined set of goals and an immediate product and don't worry about whether it's perfect. Get it out. We first have to have a good relationship with the people who are doing the typing and the integrating of it. I found that in setting up these meetings, the best thing to do was give them a framework. It doesn't have the pieces people have written, because they haven't written them yet. But you have a framework, and the typist sets us up on the computer and knows where the things come in and just puts them in those spots in the framework. They come up with a report that doesn't have to be sorted. That was a breakthrough for them, and they got terribly spoiled by it.

ASPATURIAN: To the extent that you can talk about this, what was your most interesting challenge in overseeing these projects and committees for nearly two decades?

TOMBRELLO: Predicting the future is always very hard, per Niels Bohr. The biggest challenge is getting people to think strategically and not tactically. That is extraordinarily difficult. We're going through a process now at Livermore with a directorate that has broadened beyond belief. It includes people working on climate, geophysics, biology. It's got environment, energy, chemistry. It's got physics. It's got *everything*.

ASPATURIAN: This is up at Livermore?

TOMBRELLO: Yes. We are being frustrated by the group that includes basically geology, energy, and climate science. They don't think strategically. And yet these are important fields, from a national-security and national-goals perspective. They're growing—they're growing rapidly, partly because this administration believes in them. The energy, the alternative-energy things, the climate things—very important. There's one extraordinary man up there named Ben

[Benjamin D.] Santer, who was a MacArthur ["genius" award] Fellow, an E. O. Lawrence award winner, and he's extraordinarily good. They've lost some of the other players who were roughly comparable to Ben. They've got to rebuild that group, because it's extremely important that they do. This is not just an issue of greens versus the not so greens. This is a national security issue. This affects how we deal with a bunch of other countries that are going to be affected by the climate. And we're going to be affected by the climate.

ASPATURIAN: It's hard getting a lot of people to understand that.

TOMBRELLO: In fact, we're in the process of figuring out how to replace the person who is running it with another person who will have more vision. That's hard, because this woman is a very good scientist and a very decent person. It is not her science that is being judged. It's not her human qualities, which are superb. She's even a good leader, if you could ignore the fact they have got to have a strategy. They've got to know why they're hiring the kind of people they're trying to hire, because these fields are going to grow.

This is also true in some of the other areas. What's the future of the weapons program? We don't have any of the weapons directorates. But the expertise in the scientific directorates is matrix-managed—like at JPL. And so because there is roughly only one scientific directorate—except for lasers, which is separate—those people are pulled out and attached to ongoing projects as the chemistry and physics experts. It's extremely important that they hire appropriately, not only for their own scientific needs but also for the programmatic needs of these other things. The committee can't do the strategy. We have got to encourage the directorates or divisions to do the strategy and we comment on it. Otherwise it would be as if the Caltech visiting committees tried to determine the strategy for a division. But they certainly should be there to comment on such strategies. This approach works pretty well at Caltech. It is not working in certain pieces of this directorate. That's the biggest challenge. How can you be strategic? Part of that is predicting the future. Not the long-term future but, you know, the five-year future.

ASPATURIAN: Even that can be daunting enough.

TOMBRELLO: It's interesting. It's extremely challenging. It is definitely in the national interest. You've also got to face, in the longer term, what is the real function of the national labs, particularly the weapons labs? The weapons program is smaller. A lot of us cheered START [Strategic Arms Reduction Treaty]. But we think there are still far too many nuclear weapons out there.

I will use that as a segue into something I've been doing in the background, particularly in the late nineties. We mentioned the ASCI program at Caltech earlier, and how it was prompted by worry about the reliability and long-term viability of the weapons in the stockpile that we're not going to be testing anymore. Could you predict their properties? Could you figure out a way to diagnose when they were going bad on the shelf? I mean, batteries don't last forever, and nuclear weapons are more complicated than batteries. They're filled with materials that are at least as active chemically as the stuff in batteries. Some of these weapons have been there for thirty years. I cannot go into the details of the fact that most of them are still good after thirty years. It's a great job of engineering product design. You couldn't buy an automobile and put it in the garage and not use it for thirty years and expect it to work. But you can reasonably expect most nuclear weapons to work after thirty years. And so, which ones are going bad? What are the things you need to do to retrofit the stockpile—the so-called life-extension programs? And then, you have the thornier question of the so-called RRW—the Robust Reliable Warheads, something like that. People like Senator [Jon] Kyl—not one of my favorites—

ASPATURIAN: He's the Republican from Arizona?

TOMBRELLO: I dislike Senator Kyl because he brings up reasons for not doing the START treaty that can be answered *only* by dealing with details that are classified. I feel that this behavior is underhanded. He knows perfectly well that answers to some of his questions involve material that you cannot talk about. And I consider that grossly unfair. He *knows* the answers to these questions, and he's raising them because he knows nobody can answer them. The public at large has every reason to believe that the things he is saying are important things. We have to deal with that, too. But I think we all agree that a much smaller stockpile of weapons that you know work provides a credible deterrent where you're still involved in mutually assured destruction. It's a dangerous world out there. The United States and the Soviet Union need to reduce the size of the arsenals—sorry, not the Soviet Union, Russia.

ASPATURIAN: Yes. Former Soviet Union.

TOMBRELLO: I'll have to tell a story in that regard. I told it in public when I interviewed Charlie Munger for the DuBridge Lecture a few years ago. Years earlier, I was at Los Alamos for the summer. A friend and I were bicycling up in the Jemez Mountains. We were talking as we gritted our teeth and climbed this particular hill. He was talking about competition, worrying about the competition getting ahead. I realized there was some kind of disconnect, and I said, "Jim, you can't be talking about Soviet Union." He said, "Of course I'm not talking about the Soviet Union. I'm talking about those guys at Livermore. We just can't let those guys get ahead of us." And I thought, "We're paying taxpayer money to compete with ourselves, and the people who are doing it have written off the Russians as being hopelessly clunky." [Laughter] A very interesting phenomenon.

It was deliberately set up that way by [Edward] Teller to motivate technological advances in the field, but it became an end in itself. Our national labs compete with one another, and we're going to have to get around that. There have got to be, in my opinion, a few hundred weapons in the stockpile rather than thousands. They still have not addressed the problem of the so-called tactical weapons, the small weapons, most of which are probably useless in any conceivable conflict.

ASPATURIAN: Are you talking now about this country or also about—?

TOMBRELLO: We're also talking about tactical weapons that are abroad, because they're not covered by START or SALT. But I think both sides agree that they're probably not as useful as one might think and that they are destabilizing. They're also weapons that could diffuse out of the system. That's the other thing about having fewer—you can guard them better.

ASPATURIAN: That's right. One hopes.

TOMBRELLO: The people who saved us in many respects were Senators [Samuel] Nunn [D.-Ga.], and [Richard] Lugar [R.-Ind.], with the Nunn-Lugar Act, which, as the Soviet Union came apart, found U.S. money to move people who were bomb designers into other occupations that paid them salaries and helped the Russians secure the weapons better. It was a good thing it got passed—very important. It stabilized a system that was very precarious. The Russians had a philosophy of worrying about people stealing weapons from the system. They were very poorly set up to prevent weapons from being sold by the people inside the system. Nunn–Lugar gave them a great deal of help in that. The two senators, who were willing to cross the aisle to work together and both of whom are very interesting people, deserve an enormous amount of credit. Of course, we've also had to deal with secretaries of energy who bordered on the absolute boundary of flakiness. The worst were in the Clinton administration. Clinton was a wonderful president, but Hazel O'Leary and the governor of New Mexico—

ASPATURIAN: Bill Richardson?

TOMBRELLO: Richardson, yes. They were the ultimate flakes.

ASPATURIAN: Well, now of course you have Steven Chu in there.

TOMBRELLO: I have a very high opinion of Steven Chu. I've known him a long time. I have a very high opinion of his undersecretary, Steve Koonin. I think we are in pretty good shape. There are detailed quibbles about Tom [Thomas] D'Agostino, who actually runs the NNSA [National Nuclear Security Administration], the weapons piece. He's not a bad person. I just think they tried awfully hard to find somebody to replace him. He was acting NNSA secretary when Steve Chu came in. I had several chats with Steve Chu about it. I gave him names. He told me he tried not only once but twice to hire those names. Some good people just don't want to do that particular job.

ASPATURIAN: Are you in contact with Steve Koonin on any of these issues?

TOMBRELLO: Well, he's a lot busier than I am, but I expect to see him in a few weeks, and I'm looking forward to it. We did have an interesting evening about a year ago. He convened a small ad-hoc committee to come back to the DOE for dinner in December—we were lucky; we got in and out without a blizzard—to talk about "Climategate." The department was very interested in our take—there were about half a dozen of us—on those hacked e-mails from the Hadley Climate Research Unit in England. That was a very interesting meeting. To what extent did the contents of the letters discredit climate science? And the answer is, They shouldn't have.

ASPATURIAN: No, not at all.

TOMBRELLO: But they're like all e-mails—they say things in a tone of voice that you do not intend to put in a publication. There's also a tendency in that community—because they are harassed from outside—to circle the wagons and become more insular than they probably should be. There's also a tendency in academic professions to not understand that particular fields may have implications that go far beyond publishing papers in journals. It involves money. It involves people. It involves professions. It involves—

ASPATURIAN: Public support.

TOMBRELLO: Public support. All kinds of things. I believe some of the climate-change people were extremely naïve. But I don't think anybody has shown any sign that there was anything underhanded going on. It's been an interesting experience. I've thought a lot more about climate science and may even have poked around at the edges of it with one of my undergrads this past year. I have also tried to advise JPL about their stuff in climate science. JPL could be a huge piece of the approach to dealing with the quantification of climate change. The design of the satellites. The operation of the satellites.

ASPATURIAN: They certainly have the technology and the data.

TOMBRELLO: I worry a little bit about separation. Livermore, for example, does climate science but doesn't have a climate model. They critique climate models. Therefore, it would be a bad thing if Livermore had its own climate model. I would like to see JPL get into the observation area. Their designs of the satellites and the observations would be influenced by what the climate models say, so they could check them, validate them, shoot them down. I think that would be made much more difficult if JPL got into their own climate modeling, and I hope they will stay out of it. I hope Livermore will stay out of it. It is hard to prevent them from wanting to put forward their own climate models, but I think it's become self-defeating to critique other models either experimentally or theoretically and have it accepted as objective if you also have your own competing models. I hope both groups will be wise enough to form strategic alliances with those that already have models, but not have their own models. The climate models are limited at best. The observations are tricky.

ASPATURIAN: When you speak of the climate models, you're talking about global warming and the oil-production peaks, and that sort of thing?

TOMBRELLO: Oh, the oil-production peaks are a separate thing. I know a little bit more about that. I talked a little bit about King Hubbert and seeing him when I was just out of college [Session 1]. I'm a great peak-oil person. I truly believe in it. I believe in the models. I don't think there is much else you can do there. I'm very much a fan of what David Rutledge is doing. He's looking at applying the same techniques to world coal production. To make a long story short, everybody worries about the amount of CO_2 you can make from burning coal, and that's a legitimate worry over the short term. But if you look at what Rutledge has done—which I think is *exceedingly* important, maybe underappreciated; I'm one of his greatest fans—the good news is that there isn't enough coal to make as much CO_2 as the extremists think, and the bad news is there won't be that much energy, either. So, since there is less fossil fuel that you can get at efficiently, it won't make as much CO_2, but we may all get pretty cold. [Laughter]

ASPATURIAN: Are you involved in any work looking at nuclear reactors as an energy source?

TOMBRELLO: Years ago, more than twenty-five years ago, I was on a national committee to look at reactor accidents. In *every* nuclear reactor accident, human error played an enormous role, if not the only role. That is underappreciated. It means that the technology is a lot safer than people think. It's a matter of training. But you do have to design the systems appropriately, and you have to train the people who operate the systems appropriately. I'm a fan of nuclear energy. I think there's a lot of hype out there now about new types of reactors, where they have great concepts and no designs. The difference is that a concept is something that looks attractive, and you put a few numbers into it, but in no way could you take that and actually make an actual power plant. You could produce a concept for $10 million, $20 million. To do a design study for something that's going to cost as much as a nuclear reactor or a reactor system, you might have to spend a billion dollars. And they haven't done that. They're going to have to think about that. This comes into the fusion problem, both for the magnetic confinement of fusion, like ITER [the French-based International Thermonuclear Experimental Reactor] and for the basically inertial systems, where you drive this thing together with an electron beam, a heavy-ion beam, or a laser beam. You have to get the energy—the heat—out of the neutrons that come from fusion; and that represents a blanket around it. And you have to use the neutrons to make more tritium to go back into the fusion process. So blanket design has a lot to do with reactor design. It's got fuel. It's got products. It's got transport problems. So the two of them are related. Our committee is supposed to look at that. And then there is the hybrid reactor, where you basically use the neutrons from fusion to drive to criticality what looks like a nuclear reactor. Those are very interesting, but only concept designs exist so far. Is it something you can reasonably start attacking now? I think it can be attacked now—just add a billion dollars and do some reasonable design work.

Then there's the role of industry in all this. The reactor program was subsidized by the government, but it was overseen by Westinghouse and General Electric and Babcock & Wilcox, who make the commercial reactors, not to mention all the foreign versions of those. Which industries will take this on? I made the modest proposal to the committee that maybe you need a new paradigm, and I know this young industrial giant named Elon Musk, who has formed a spacecraft company and probably done more things than many national rocket programs. Granted, he's getting funding from NASA, but his program was developed separate from NASA and has a whole new business model. And he's also developed the Tesla car company, It's too soon to tell how successful that will be, but I said it's worth listening to a visionary who might attack this problem in a completely different way from Westinghouse or GE. A modest proposal. Since I know Elon and I know he's a fan of fusion, but he hasn't picked a winner yet, I think it would be interesting to bring him in and hear what he has to say. It's a long shot, but an interesting one.

ASPATURIAN: Do you back nuclear—fission or fusion—over solar energy as a solution?

TOMBRELLO: I am a believer that there are many niches in the energy market. If I were building a remote cabin in a place where I would have to run in a power line from five miles away, it would *clearly* cost me a lot of money to tie in to the grid and buy commercial power. I would think very, very seriously about making this house completely independent and probably powered by solar. There are niches—fill them. Back when I was a kid—and before then, too—a farmer out in the middle of nowhere, where there was no electricity, could buy a windmill kit. Came in a box. You put it up, and you had a little bit of power and you pumped water into your stock tank. That's a niche. Sears and Montgomery Ward filled it with these windmill kits. I'm a great believer that—and this is an evolutionary biology model—evolution occurs when, usually in some small environment, there's a niche to be filled, and something evolves into the niche and exploits it. Find the niches, encourage people to try to fill them with biofuels, with wind, water, tide. And for big power, nuclear reactors of one kind or another. It's a compact power source. The density of power for solar cells—folding in a lot of factors—is, let's say, 100 watts per square meter. You know, I'm folding in winter, clouds, day and night, and stuff like that. But, you know, that's not very dense power. To get up to 1,000 megawatts electrical at 100 watts a time requires a big piece of real estate. So I'm not against it. I'm just saying there are niches, and some will be filled by dense, large power, big power. Some will be filled by a single windmill or a windmill farm. Some will be filled by solar-cell farms or thermal solar. I'm a great believer—[laughter] go back to Mao and say, "Let a thousand flowers bloom."

ASPATURIAN: Speaking of Mao, are your committees—both in terms of energy, climate change, and weapons—international? Are you working with your counterparts in other countries on any of these areas?

TOMBRELLO: On the nuclear reactor accident thing, we worked with a lot of different people. Not with the Chinese, because, remember, it was twenty-five years ago. We knew what was going on in the Soviet Union, and just about the time we wrote the report, Chernobyl happened; so we clearly were plugged into a lot. Often there are Brits. There's always a special relationship there. Occasionally, and on the laser stuff, I suspect we'll have some people from France that we talk to, because they're building a big laser system that's very like this big NIF [National Ignition Facility] laser at Livermore. In fact, they're using materials for the laser that were developed and caused to be manufactured by Livermore. So there is an international component.

ASPATURIAN: How about countries that are turning into, you know, scientific powerhouses at some level or another. Singapore, Israel, the PRC. Well, it doesn't really call itself the PRC anymore—China. Parts of India.

TOMBRELLO: Well, I think of it as the PRC, because, after all, there's Taiwan.

ASPATURIAN: That's true.

TOMBRELLO: Not much collaboration, but we try to figure out what's going on. On the weapons game, of course, Israel is, officially, an enigma.

ASPATURIAN: Well, yes.

TOMBRELLO: And I have concerns about Israel. It's the hope of democracy in the Middle East, but at the same time the current government is not one of my favorites. I'm not a fan of [Israeli Prime Minister Binyamin] Bibi Netanyahu and certainly not of [Israeli Minister of Foreign Affairs] Avigdor Lieberman, whom I consider seriously destabilizing individuals, and I think Obama's handling it—

ASPATURIAN: As well as he can?

TOMBRELLO: —in a mediocre fashion.

ASPATURIAN: Oh, really?

TOMBRELLO: Yes. Can it be handled better? That's another question. I'm concerned that the American Jewish population is backing a single party in Israel that does not speak for even a majority of the Israeli people.

ASPATURIAN: Although there's a quite large split among the American Jewish community on this issue.

TOMBRELLO: I know. I've noticed that, and I wish them well. Because it would be a good thing if they were not just parroting the line of the Likud Party, which I feel very uneasy about. That does not automatically make me a fan of Labor, but I would like to see [Israeli Opposition Leader Tzipporah] Tzipi Livni move forward. I would like to see her run Israel, just to see what will happen. Seems like an interesting lady. Once she's in, I might not like what she does. If I were in that country, I might well be doing exactly what they're doing, and I try to temper what I say. But what they're doing is not necessarily in the interests of the rest of the world. But then again, let's not talk about the Muslim countries, because then I really get concerned: what Iran is doing; what Iraq was doing. The country I worry about the most of all in the whole world is Pakistan, and I have worried about them for more than two decades. They have been taking nuclear weapons technology that they were given by the Chinese and selling them on the street to all concerned. And this has been going on for a long time. They traded nuclear weapons technology to North Korea for rocket technology that the Koreans had developed. They were basically selling to Libya, Iraq, Iran. You name it, they were doing it, and while they may want to blame it all on Mr. Khan [Pakistani nuclear scientist Abdul Qadeer Khan], I think it was government policy in Pakistan and may continue to be.

Pakistan is the most dangerous country in the world. I've said this for many, many years. There are countries that are, you know, quiescent now. When the ANC [African National Congress] took over in South Africa, the previous government destroyed the nuclear weapons they had, and they destroyed the capability of building them, but they did not destroy the ability to process U-235, nor did they get rid of the fact they have a lot of U-235 that's perfectly decent material to make weapons out of. So you might say there is a sleeping giant in the nuclear business in South Africa. Libya got out of the game as a trade for something else—Lockerbie, I don't know.

ASPATURIAN: Maybe Qaddafi just got tired of it.

TOMBRELLO: Or help from the West in producing oil. The whole thing is quite unstable and has gotten more so. I remember writing a position paper at Livermore—I guess some of it is classified, but the most of it certainly is not. [Francis] Fukuyama was talking about the end of history, and my opinion, back at that point in time, was that it was not that way. The Soviet

Union and the United States had every reason to keep everybody under very tight control. We prevented a lot of things from happening just by sheer intimidation. It was as if you had a pressure cooker and you kept the lid nailed down tight. And my prediction in 1992 was that we're no longer controlling the pressure cooker, and it's building up; and we don't know where everything is going, but it's not just two players anymore, it's everybody. That's when I suggested that Livermore get into another line of business. That even if peace were coming, we still want to read other people's mail. I said they should get into the surveillance business, intelligence business. Widgets for the intelligence crowd. I made a basic mistake in that. I looked on the CIA, with its enormous budget, as being a monolith to whom you could sell all sorts of things. But I got it completely wrong. The CIA is huge. It has a huge budget. But you will sell a widget in one office, and the guy next door couldn't care less; he wants a different widget. So it became a much more interesting sales job, and the people that worked with that program at Livermore were geniuses at marketing something that was a marketing nightmare, which is equipment to help you determine proliferation and things like that.

ASPATURIAN: Well, it's very necessary, isn't it?

TOMBRELLO: It's very necessary, but it was a different marketing challenge than I had anticipated. It was not what I expected at all. I was right, but it was a much bigger challenge. I'm pleased that Livermore made it into about a $300-million-a-year enterprise.

ASPATURIAN: Anything else you'd like to add to this particular session?

TOMBRELLO: No. I think I mentioned that the ultimate thing is, What is the ultimate role of, not just the weapons labs in the future but all the national laboratories? What is their long-term function? Do they have a vision? The origin of the Argonne National Laboratory was the reactor program, the Light Water Reactor Program. Well, they don't do that anymore. I'm not just picking on Argonne National Lab—I'm picking on Brookhaven; I'm picking on Livermore; Sandia; Los Alamos.

I think that's probably it. I think there's not much more to the national labs. We can do the division tomorrow, starting on that.

THOMAS A. TOMBRELLO

SESSION 8

December 29, 2010

ASPATURIAN: We are primarily going to talk today about your years as division chairman. That would be chairman of the Division of Physics, Mathematics, and Astronomy, from 1998 to 2008?

TOMBRELLO: That's right. Ten years and one month. You carefully stated the name of the division, so I have to tell a little story about the name of the division. I was at an event that was organized by Caltech's Development group. They had a little program of the presentations that would be made at dinner. I was listed as the chairman of the Division of Physics, Mathematics, and Astrology. And so if there is a theme to all of this, it is how you can work around the limitations of Caltech's Development group and raise, in the process, a third of a billion dollars over ten years.

ASPATURIAN: When was this?

TOMBRELLO: This must have been probably a year or two into my term—probably about the year 2000. It was not a joke. I'm sure they thought it was correct. The event was in the Athenaeum Library, and I read it to the guests. I said that I thought it was a very good little side business for the division to raise money, because, after all, who could do better astrology than somebody with two 10-meter telescopes on the top of Mauna Kea?

ASPATURIAN: Quick save!

TOMBRELLO: It wasn't a save; a knife job on Development is what it was!

Anyway I got in at the end of the summer of 1998.

ASPATURIAN: What were the circumstances of your appointment?

TOMBRELLO: Well, of course the candidate is the one who knows the least about it. I knew a little bit about the [search] committee. I believe that the committee had two concerns. One, they needed to get things going in the division. Two, they knew that times were likely to be tight. So they felt that they had to have somebody running the division who could say no. I do not think they put forward any other names. I don't think my choice was particularly popular with David Baltimore—but, anyway. They sold it, and Koonin and Baltimore approved the appointment. I took over.

ASPATURIAN: Were you surprised?

TOMBRELLO: In a way, yes. I had been asked several times, in previous searches for chairs, about whether I had any ambitions in that regard. I said, "Look, I have had the luxury"—or taken the luxury, the liberty—"of saying exactly what I've thought all the time." I knew that was not popular, and I knew that was a career-limiting attribute, particularly to be something like chair of the division.

I think of myself as a reasonably sane person, and I knew perfectly well that having taken such a liberty, or a whole series of liberties, reduced the chance of being division chair to pretty close to zero. I told several different search committees that. I think they all believed it. Yet I knew I had been a candidate in the previous search, where they put forward four names. I thought that the odds I'd be chosen those times were small, and they were zero. I thought—when I was interviewed in 1998—that the chances weren't very high then, either. I thought the division was a bit desperate, because they knew there were priorities that needed to go through. I think we mentioned that the division staffing committee I set up had arrived at a set of priorities. The first was to find money for endowed postdoctoral fellowships. The second was to try to hire Ed Witten, the best elementary-particle theorist they could think of. And, perhaps dead last, to get that astrophysics building that had been kicking around since 1966. I was certainly prepared to try to get those things through. We had a new president, Baltimore. I figured there would probably be a fund-raising campaign. There had been no announcement, but I assumed it was out there lurking. I didn't think things were going to be quite as bad financially as the division thought they were going to be, although the first thing I encountered—I mentioned this yesterday—was that out of a budget of millions, my predecessor, Charlie Peck, had left me a discretionary $19,000. I cried foul to Koonin. He laughed at me and said, "You're good with money. Let's see what you can do." Because of the rather primitive nature of Caltech's accounting system, you could sort of generate virtual money. It also helped to renormalize the expectations of the division. Having no money does a good job of that, because you can say, "There just isn't any money."

The first thing was to try to get a few things moving. One was to find the money for the endowed postdoc positions. One of our trustees, Walter Burke, had been the head of the Sherman Fairchild Foundation. His daughter was now running it, but he was still a major force. I'd just been made chair, and Walter wanted to have dinner with me. We went to the Athenaeum, chatted about any number of things, and took a liking to one another. Walter basically said, "What are your priorities?" And I told him about the fellowships. He said, "You got a choice. You can have ten million dollars for fellowships, or we could probably find ten million for the [astrophysics] building." I said, "The priorities are very clear. We want the fellowships."

The procedure was, it had to be looked at by the Fairchild board. They sent out a member of the board whose name escapes me at the moment, but I think he'd been president of a college until very recently—a physicist. We made a presentation to him with Roger Blandford. We had discussed the nature of the presentation, but Roger was a genius at it. He had some viewgraphs—people were still using viewgraphs then. He put up a viewgraph that sort of said, Where are they now?—people who in the past had had Caltech named fellowships in, for example, theoretical astrophysics. It was a *Who's Who*. It was people running major facilities. It was people running laboratories, centers. It was wonderful. He had a page full of them. The person from the foundation board said, "But what about the unnamed postdocs you had? Let's do a control on that." Roger, with a smile, takes the first viewgraph off and puts another one up. And there they are. They're doing fine, they're certainly not chopped liver, but they're not like the first list. The guy says, "You've made your point." Really, the whole thing was over then. We were going to get $10 million. So I started off on a roll; I'd gotten that money.

ASPATURIAN: This was 2000?

TOMBRELLO: That was probably in early 1999, because there is a second verse to this story. I was also wooing a man named [Charles] Cahill. Chris Yates in Development, the lawyer, had found him. Cahill wanted, potentially, to give his entire fortune to Caltech if he saw a vision. I gave him a vision of a building that would change the sociology of astronomy and astrophysics on campus by putting together groups that were currently spread over several buildings. It was going to make one tribe out of two tribes or three tribes. He liked that. And he liked me. So I figured I was going to get some money for that building. That was in the works. The reason 1999 is a date that's important is because, you remember, we were in the middle of the dot-com bubble.

ASPATURIAN: Yes, I do remember.

TOMBRELLO: So I was going to get $10 million from Fairchild in two $5-million pieces over the next two years. But by the time the first money was due, the bubble had burst, and everybody had been heavy in dot-com stocks, and certainly the Fairchild Foundation. I got a phone call, and perhaps a letter, from Walter saying, "Look, we've got a problem. We're going to give you the money, but you're going to have to be patient. It's not going to come exactly when you think it should." I thought there was something I could do. I said, "Walter, that's not a problem. But to show we have such faith, we're going to start this program when we were going to anyway." I got Caltech—Koonin particularly—to back the idea, and I wrote out a business plan of how we could fund the first choice of postdocs—not as many as we would have had otherwise, but a few—to show the foundation that we believed they were really giving us the money and to show them that we were going to get some really interesting people here. That was a *very* good investment. It was a good investment in Walter Burke, who likes backing winners. There's going to be a little sequel to this story, and I'm going to tell it out of sequence in just a minute. And the money started coming in. The stock market started rebuilding it, and we did get the $10 million. We repaid our debt to Caltech with a little bit of advance funding, which was no more than half a million dollars. It may even have been less. The whole thing was going forward.

Let me give you the second Walter Burke verse, where we have to sort of speed forward roughly ten years. This is probably the summer of '09. I'm now not in office, and Andrew

Lange is PMA chairman. Peter Hero has just come to run Development. I'm sitting on my lanai, in Kauai and talking to them on a cell phone.

ASPATURIAN: Three-way call.

TOMBRELLO: Three-way call. They want to talk to me about Walter Burke. I said, "Keep in mind, Walter Burke likes winners. What are you going to propose to him?" They said they were going to tell him that the endowment, including the endowment for the Fairchild postdocs, has been hurt. Would the foundation give us money to make up the difference?

I said, "That doesn't fit with Walter's personality. You are going to tell Walter that you have lost 30 percent of his money *and* that you want him to make it up?" I said, "I don't recommend this. Walter likes winners. Go to Walter with something new, and you will probably get it. Go to him with an admission of failure, and he's going to politely say no." And, of course, that's exactly what happened.

ASPATURIAN: Really?

TOMBRELLO: Yes. They just didn't listen. They thought: We're making a reasonable request of the foundation to continue a program they started. But they didn't take into account the personality of the man running it. I just wanted to jump ahead to this story to tie it to Walter's personality.

Anyway, I'd met my first challenge and I started the Witten negotiations. We're back in 1999 now.

ASPATURIAN: Talk a bit about Ed Witten for a moment.

TOMBRELLO: Well, first, he was an interesting case. He was clearly the top of the string theory stuff. Before that, he had been on top of a number of other things. He'd won a number of prizes—not the Nobel Prize but other prizes. He clearly was one of the best people in this field, and it was not a crazy thing to go after him at the Institute for Advanced Study, and there was potentially a way to attack it. His wife, Chiara Nappi, was also a theorist, roughly the same field, but in a non-professorial position. I thought, OK, I don't think I can get her through Caltech.

But maybe if we form a strategic alliance with, for example, USC—which also had a small string group—we could create something there. Now, the whole deal was an interesting package. I was doing it with money from Gordon Moore—at least a couple of million dollars.

ASPATURIAN: So he was on board with this.

TOMBRELLO: Absolutely! We remodeled the Downs Lab. We moved a bunch of the astrophysicists to sub-basements, where they had better labs. We moved the shop. We created a floor for these people in string theory. I had by that time hired three young string theorists: [Anton] Kapustin, [Hirosi] Ooguri, and [Steven] Gubser. Kapustin and Ooguri are still here. Gubser, because of a two-body problem, ended up going back to Princeton. Still I'd made a great leap forward, hired some people. Got this deal running with USC—a joint theory institute. Somewhere in there I got a professorship offer out of them for Chiara. David Baltimore thought, "It's all over." I wish it had been. We had done a great job. Witten and Chiara spent two years here; everything worked very well. But Curt [Curtis] Callan, who was chairman of the Physics Department at Princeton, saw his own opportunity, and without asking the Institute for Advanced Study he created by magic, out of nothing, an offer for Chiara at Princeton. That meant they did not have to leave Princeton, and I lost.

However we got a very good string theory group started. We got some space remodeled. We were put on the map, basically, by trying to do it, even though we failed. I don't like failing, but we really had done a good job with it.

ASPATURIAN: Do you think Witten was really such a loss?

TOMBRELLO: Don't know. I do know that trying to hire him was important to establish this credibility with the division. And to make it clear in the outside world that Caltech was really willing to play major league ball. We went after him, and people noticed. It made recruiting easier, rather than harder. One of the Princeton students who was involved in this was Sergei Gukov. He is one of the best and the brightest, and I ended up hiring him. So all that led to a rather powerful string group. You might say the string quartet—quintet, if I count John Schwarz [Harold Brown Professor of Theoretical Physics], who was the founder of the field. It's an interesting group. It's a bet on futures. There is a bit of a joke about that, because in '99, while

Witten's here, Zewail wins the Nobel Prize. There's the usual celebration across campus. There's Champagne everywhere you go. I met up with the "stringers." The "stringy" ones ask me, "Do you have any other Nobel Prizes in mind?" I said, "Oh, yeah! There's one sitting on the corner of my desk for you guys in string theory." I said, "I'm just ready to send it in, any minute. But all you have to do is give me a number. I mean calculate something that will nail the theory down, like the mass of the electron—you know, something. What got Niels Bohr the prize, really, when he did the first stuff on quantum theory, was that he could calculate the Rydberg constant. It was no longer a constant; it was something he calculated. You guys give me a number the equivalent of the Rydberg and, my, you're off to Stockholm in the twinkling of an eye." They all tittered, and I said, "But you've got to bring me the number, guys. Because it's not going in without a number." And they're still waiting for the number.

ASPATURIAN: String theory is an interesting field.

TOMBRELLO: It is a beautiful theory. It smells very good. I was certainly willing to make a big bet on it. But it's been in a quiet period for a while. They still have not figured out a way to calculate things. It's proved to be more complicated than they thought it would be.

ASPATURIAN: One wonders if it is perhaps a more complex approximation of something else.

TOMBRELLO: Anybody's guess. So anyway, I had met the first goal and made the start of an honest try on the astrophysics building. With Gordon's money all things were possible.

If I may jump ahead a few years—since I'm on the building—I was at another dinner with Walter Burke. The stock market has recovered a bit now from the dot-com bust. He says, "We really liked what you did with the postdocs. I told you when you took the ten million for the postdocs that it was that or the building. How'd you like ten million for the building?" I said, "Walter, you're making me look awfully good." So, there was another $10 million. And he said, "We don't even care if we have our name on anything." I said, "Walter, you're making me even better. I can sell some of this stuff twice now." Walter's a gentleman, and it's fun working with him. But I will tell you, he backs winners. Since I'm on Walter, I will tell one more story. We were at an off-site meeting. Walter is a man of very strong opinions. He said, "You know, it really bothers me that you have to go with your hat in hand to those fund-raisers for things. Would a little bit of discretionary money—you know, that was just yours—help?" I said, "It wouldn't hurt, Walter. And I will always give you a very good accounting of how I spend it." One of the ways I spent the discretionary money some years later was when one of our donors, John Robinson—whom I mentioned earlier—was dying at Pebble Beach. At the time, he was interested in cosmology, and I used tiny bits of Walter's money to send Kip Thorne, Marc Kamionkowski, and Andrew Lange up to entertain him. And they loved him. He loved them. And it was entertaining. There was a little bit of an upset with Walter, because at our next meeting he said, "Look. You're not spending that money fast enough." I said, "Walter, you gave money to a Depression kid. I've got to squeeze every damn dime until it squeals." He said, "You know, I'm not going to live forever. You spend it, because once you spend it, I'll give you some more." [Laughter] So that's Walter. He loves winners, and I appeared to be a winner for Walter, and he loved it.

So let's now go back. We're in early 2000. The Cahill building is now moving forward. We were going after a preliminary design. We had thought about a price tag of $30 million. Development had played very little role in any of that. Things are moving forward. I think I've told this story already, but we'll repeat it, because it fits. We're at an off-site trustees' meeting in Palm Springs. Ben Rosen, who had been a Caltech undergrad, has just replaced Gordon Moore as chair of the trustees. He came up to me and said, "I'm really disappointed in you. We announced this fund-raising drive this morning and you haven't given me anything yet." I said, "Give me five minutes. The list is in my room." He thought I was joking. But I got it for him, and he looked at it and was impressed. It was a big wish list. We ended up getting a lot of it. However, you know, the whole business of raising money is not a case of telling people what you want and getting it. Some things were really worth having, like the CHICOS [California High School Cosmic Ray Observatory] project idea that Bob McKeown had of putting these cosmic-ray detectors in local high schools. That was, I thought, a winner. We didn't get any money for it. Roger Blandford left [2003] and went to Stanford—I think I could have kept him at Caltech if I'd gotten any money for numerical astrophysics. I ended up getting a lot of money later for numerical relativity for Thorne, mostly from the Fairchild Foundation. But Walter is a true believer in Kip Thorne. He is, as I say, a very sound man with very good judgment. There were a number of other things I just struck out on.

There were many things that were unexpected, and I'm going to start with one of those, and now we have to go back to the fall of 1998. Koonin and I and a number of others are celebrating some anniversary—maybe for Owens Valley—and he says, "Well, when are you going to produce a proposal for the next big telescope?" I said, "Hey, boss. That's a great idea. I think we're going to start on that this afternoon." So Wal [Wallace L. W.] Sargent [Bowen Professor of Astronomy] is up there, because his wife, Anneila, is director of the observatory. I said, "Hey, Wal. How'd you like a big telescope?" That started what has become the TMT Project. Koonin and I started it. We had $1 million from Gordon Moore. Gordon was *very* generous, has always been very generous. But he hadn't picked that particular project. It was money he'd given Caltech, and Koonin gave it to me and said, "Get this thing started." We put some other money together, and we got started on that. After about a year, we discovered that Caltech just couldn't do it alone. Caltech did not have much expertise in optical telescope building, although lots of expertise in observing on the telescopes. So we pulled in the University of California and then added other partners. But that brings up strategy from the point of view of a division chair. Except for this $1 million in seed money, there was really no movement forward of trying to get any TMT funding out of the campaign that was getting started. Baltimore was not convinced it was a project worth funding. Something interesting happened. Bud [Albert D.] Wheelon, one of the trustees, had said to me privately, "You know, the trustees would really like to go to some off-site things, rather than just always meet at Caltech or Smoke Tree or wherever." I said, "Oh, yeah? You have anything in mind?" He said, "Well, we could go to LIGO." Then the penny dropped, and I thought, "You know, Bud, I have a different idea."

So I went to Baltimore and said, "I've heard from Bud Wheelon that the trustees would like to go other places. What if we had an off-site trustees' meeting on the Big Island of Hawaii at Mauna Kea?" He said, "That's a great idea. But what would they do?" I said, "Well, we're close to the telescopes, so I guess I could arrange that people would give some talks, and we could have some tours of the observatories. We've got the sub-millimeter observatory. We've got the Keck Telescopes. You know, we could really make it into an interesting meeting." He said, "Oh, that's good. We'll do that." I thought, You don't know what I'm doing, and I'm not going to tell you. So, of course, what did we have? We had talks by Ed Stone about how the

Kecks had gotten started. Then, we had a talk by Jerry Nelson, the inventor of the Keck telescopes.

I had a captive audience. I had all the trustees, but I had one trustee whom I had targeted—that was Gordon. I was going to educate Gordon about big telescopes and the future and the vision of building the next big telescope. The start of it was that off-site meeting at Mauna Kea, and I had definitely planned it as the beginning of making this telescope real—not at the million-dollar level. Development played no role in it. They had no idea what I was doing because, frankly, Development didn't think, after having just finished the Kecks, that there was any money there for more telescopes. I thought: What do you guys know? I don't know either, but we have to start. The trustees had a wonderful time. Gordon had a wonderful time. We had all sorts of interesting adventures with the telescopes, with rides up the mountain, parties.

You don't have to be overt about fund-raising. You can start it as an educational program, knowing it may come to nothing. But unless you start it and show people the point, you're never going to get anywhere. I think the classic Development thing would have been to go to Gordon with a proposal and ask him to say yes or no.

ASPATURIAN: I believe that's correct.

TOMBRELLO: And the answer you'll get is, It's much too soon.

ASPATURIAN: So your approach was a classic example of show, not tell.

TOMBRELLO: Take your time. Be patient, but always have a framework in which to do things. There are a lot of people who think strategy is a rigid structure. Strategy is a framework—a framework in which you will have some serendipitous event, and suddenly things are in this framework in a different order than they were. But without the framework, without the strategic vision, you don't know where to put things. You don't often recognize opportunities. It wasn't just the TMT; there were a number of areas that were like that. Frameworks that were not fleshed out, but as time went on you had things happen, and you could attach objects to them, or attach directions to them. The flow chart might change a little bit because of something that happened, but you always knew where you were trying to go. You had a pretty good idea of how much money was involved, and we're talking about *a lot* of money for the TMT. That was the key—the hallmark—to my being chair. I think I brought strategy—maybe some will say low cunning—to the job.

I'll give another example—this is something that has not been funded yet but I hope will be. I needed an outside partner. I was the first chair of the board for CARMA [Combined Array for Research in Millimeter-wave Astronomy]. CARMA was the combined Berkeley–Caltech project to build a radio telescope observatory up in the Inyo Mountains and to use Owens Valley as the base camp. Through blind bad luck or good luck or whatever, I was the first chair of that, and I wanted an outside advisory committee. I got John Carlstrom, who used to be an astronomer at Caltech. I adore John. I'd love to bring him back here. But I wanted somebody else, too, and someone said get Martha Haynes. She's a radio astronomer at Cornell and responsible for the Arecibo Observatory in Puerto Rico. I'd never met her, but we got on the phone. We must have talked for two hours. It was a meeting of the minds. We talked about CARMA, mainly. And then she started asking me about other things in radio astronomy at Caltech. I said, "Well, I've got this observatory on top of Mauna Kea. Now, it's an aging observatory, but it has a ten-meter sub-millimeter telescope, designed by Robert Leighton. It's a very good telescope, but I have to start getting that group thinking about where we go next. They are superb instrumenters—Jonas Zmuidzinas [Kingsley Professor of Physics] and Tom [Thomas G.] Phillips [MacArthur Professor of Physics]. They build beautiful stuff. This is one of the world's great groups, with one of the world's great facilities." She said, "Well, we have some ideas at Cornell, about building the next generation of those." And I said, "Well, hey, Martha. Why don't we get together?" And so CCAT [Cerro Chajnantor Atacama Telescope] arose out of that conversation. Again, if you have a framework and somebody says the right word, you know what the next step is in this journey of a thousand miles. At that moment, CCAT was born. In those days, it was the Cornell–Caltech Atacama Telescope. I found some money. Jakob van Zyl, at JPL [deputy director for the Astronomy and Physics Directorate], found some money. Cornell had some money. We started a concept study, followed by what seems to be now a design study. We still haven't raised a lot of money. I hope to get some from Caltech. But now we're in this recession, although I do believe still that Caltech will put up some money for it.

Let's talk about TMT some more, because, again, it's strategy. At the time, my former student was the provost. I talk to him. He's a believer. He and I were founding fathers, you might say, of the TMT. It was called CELT—the California Extremely Large Telescope—then. I think that was Wal Sargent's name. I had some money. The University of California had a little bit of money. I said to Koonin, "What will it take to move us forward?" He said, "You've got to get this thing evaluated." I said, "What if I'm willing to produce a concept study—I don't have enough money for a design study—a concept study, and we get an outside committee to review it as if it were a serious project?" He said, "You have the money, let's do it." And so we did. We worked for almost a year to produce something called the Green Book, which was a very attractive book. It would have been nice to have had the Green Book before we had that meeting at Mauna Kea, but it was pretty close in time.

We held the next meeting up in Oakland. We had an outside committee of considerable repute. This was a *Who's Who*. We picked an interesting guy to chair it—Ed [Edward] Moses. Koonin knew him because Moses had saved the National Ignition Facility after it fell on hard times. They had an associate director, Mike [E. Michael] Campbell, who turned out to not know what he was doing—or maybe he did know what he was doing but was some parts fraud or self-delusion. Anyway, they were going to build this big laser for plasma physics—plasma fusion—research for $800 million. At last count, the cost is probably closer to $3.5 billion. Anyway, Moses is one of the world's great project managers. Clearly, he was running NIF very well. Previously, he'd run AVLIS [Atomic Vapor Laser Isotope Separation], the laser isotope separation facility at Livermore, which was a brilliant piece of work, probably harder than NIF in some ways. This is a tough guy whom you're not going to sell garbage to, and he gave us a lot of credibility. He had a strong committee with him, and they gave the TMT Green Book an A. They liked the proposal. That certainly had its effect on David Baltimore. I was going after, I hoped, $25 million from the Moore Foundation gift of $300 million to Caltech. Baltimore was still skeptical, because he thought that UC plus Caltech was still too small a footprint. But when Richard Ellis [Steele Family Professor of Astronomy] and I found another partner, the Canadians, it became clear that we had a big enough footprint. Baltimore authorized us to go after $17.5 million from the Moore Foundation, and the University of California also went after and got the same amount. So we now had $35 million.

ASPATURIAN: Can I back you up for one moment? Was Richard Ellis brought in around this time specifically with the TMT in mind?

TOMBRELLO: Not exactly. We'll have to step back to 1998. I'd been on the committee, because I wasn't chair yet, to hire Reinhard Genzel from Europe to come to Caltech. But he eventually backed out of negotiations. I asked Roger Blandford, "What do we do next?" He said, "You ought to look at Richard Ellis." I did. At the time, he was at Cambridge. He had previously been at Durham University, where he had made a name for himself. I got him over here. Realized he was ambitious and hungry for a big telescope. Well, I had two big telescopes. The British were not showing any signs of life on big telescopes. I also had a plan for an even bigger telescope. I would have wanted Richard no matter what, but Richard resonated with the telescope project. He would immediately have a piece of Caltech's time—a lot of time—on the Kecks, and he had the promise of an even bigger telescope. So, yes, I didn't go after Richard with the idea of TMT, but TMT went after Richard Ellis for me, you might say. Richard wanted something big. People have said, "Oh, you're going to lose Richard Ellis." I tell them, "Not unless they have a bigger telescope than I have." He's a good guy. We hit it off quite well, and I think we did some nice things together. He replaced Wal Sargent as head of Palomar. He had a vision that matched the vision that we were already working on, and it has gone very, very well.

Richard has a different style than some of our other great observers at Caltech, and we have some really truly great observers at Caltech. Wal Sargent's one of them; and of course Maarten Schmidt was as good as we ever had. We have the former student of Wal Sargent's, Chuck [Charles C.] Steidel [DuBridge Professor of Astronomy], who has clearly become one of the giants in the field. Chuck is an *amazing* man. I keep thinking of him as a young man; he's probably fifty now—still young, compared to myself. He has the ability— There's this old joke that Ronald Reagan used to tell about optimism. A kid is given the job of shoveling out a stable. And the kid is happy as a clam; he's whistling; and somebody says, "You're shoveling shit, kid. Why are you so happy?" The kid says, "With all this horse shit in here, somewhere there's got to be a pony."

All I can say about Steidel is that I would be willing to bet that he can walk into any stable and pick up the one gold coin that's hidden somewhere by just reaching down and picking it up. He has an absolute genius for doing the right observations. People talk about data mining. If you've got Chuck Steidel, you don't have to mine data. He just finds the soft spot immediately. He's a genius.

ASPATURIAN: What are your thoughts about some of the other observational astronomers on the faculty?

TOMBRELLO: Shri [Shrinivas R.] Kulkarni [MacArthur Professor of Astronomy and Planetary Science] is a genius. He has a completely different style. He's across the map. He does a little bit of everything. He is clearly one of the world's great astronomers with a completely different style than the others. [Stanislav] George Djorgovski is the kind of astronomer who—I think he's frustrated by being in the same group as Steidel. Steidel would just reach down and find the gold coin. George would have to shovel through all of it and might then find the gold coin. It's got to be frustrating to be around people like Steidel. Just totally frustrating. He's just too good. It looks like luck, but it can't be if you do it over and over again.

So, anyway, now we're moving TMT forward.

ASPATURIAN: Right. You've got two $17.5-million commitments.

TOMBRELLO: And we've got a commitment for some matching funds from the Canadians. Clearly, we had what we thought was the beginning of a critical mass. We had money. We had the design study started. Now we have to sort through other big proposals. The next thing was a nanotechnology center. We could clearly get money for that, but when Dave Tirrell, chairman in chemistry [Division of Chemistry and Chemical Engineering], and I saw the proposal for it, we thought it looked like an entitlement proposal. The last thing we wanted was to have a project where the money came in, got spent, and was gone.

ASPATURIAN: The proposal came from whom?

TOMBRELLO: It came from PMA, engineering, chemistry, and maybe biology.

ASPATURIAN: So it was a very interdisciplinary proposal.

TOMBRELLO: Yes, but Tirrell and I didn't like it, although we liked the idea of the center. We thought that we needed to get to a situation in nanotech where it isn't just each person having one little widget that they do something with. We needed a facility where there were lots of different pieces of hardware that they could use together, rather than each one staying with their own little electron microscope or e-beam writer or whatever. We wanted a combined facility that's up to date, competitive with everybody else, and with a strategy for replacing these things. Because, again, if you have a bunch of new equipment, in five years it's a bunch of old equipment. It's not competitive anymore. How do we set it up? Tirrell and I were not entirely popular, because we eliminated the entitlement aspects—paying salaries, having seminars, and stuff like that. We basically said, "Look. Buy equipment. We will have a certain number of years for which you have service contracts and technicians to run these things. At that point, you better figure out how to support it, because it's not clear it's going to carry through to the next fund-raising drive." We got roughly $25 million from the Moore Foundation, I think, and $7.5 million from the Kavli Foundation.

So we have a lot of money to begin with. And we started with Mike [Michael L.] Roukes [Abbey Professor of Physics, Applied Physics, and Bioengineering] as the director—but with a plan that the directorship would rotate and that a council would choose the next director. The next director was [Axel] Scherer. And at the moment Roukes and Scherer are co-directors. Scherer is a genius with the equipment, knowing what to do, how to keep it running, how to look to the future. Roukes is Mr. Outside, who knows how to go out there and sell it. I think it turns out to be a good combination—Roukes was in at the right time at the beginning, and Scherer came in at the right time when you try to build it up, and now we have put the two of them together. Now they have gotten some more money from the Moore Foundation, and I think they may get more from the Kavli Foundation. And basically the cash-flow situation with users is quite good. If they're not paying their whole operating expenses, I think they're very, very close to doing it. It was a very good model, and I think David Tirrell and I deserve the credit for establishing a credible model that not only got this thing for them but gave them a path by which it would be self-sustaining and would go on forever, one hopes.

Another thing I did was that JPL was interested in hiring scientists. I said to Elachi—he became JPL director in 2001—"Let's do an experiment. You want scientists. I've got scientists. You're going to need a standard of comparison when you starting hiring your own scientists, and you don't have that now because you don't have any scientists." I said, "I will propose that we put some joint appointments in of people who are really good. They will give you a scientific baseline. And anytime somebody wants to hire someone, you can say, 'Anybody we hire has got to be as good as this joint appointment.'" First person was Jonas Zmuidzinas; then it was Andrew Lange, Tony [Anthony C. S.] Readhead [Rawn Professor of Astronomy], and Tom [Thomas Allen] Prince [professor of physics]—four distinguished Caltech scientists who not only performed well at JPL but really showed what a great scientist is. I'm a great believer in comparisons. I do not believe that institutions automatically know whom to hire, and this was a way of establishing a basis for that.

The reason I introduce it at this time is that they needed somebody to head the Microdevices Laboratory at JPL. You might say it's like the Kavli Center on campus, but at JPL. I talked to Jonas Zmuidzinas about it. He was willing to accept this job, particularly if his duties were mainly technical—in other words, choosing the right equipment, choosing the right people to use the equipment, deciding how to keep the various projects going. Jonas and I decided that if the Microdevices Lab wanted something, it shouldn't overlap with what the Kavli Center would have. We wanted the two labs to find a working relationship so that we could share the future development. So that anytime you got a new e-beam writer, you got only one, and you decided whether it was going to be based on campus or JPL, and the other lab got to use it. You work out a barter system, so that money isn't constantly being moved back and forth, and that allowed people to use the best equipment everywhere. There was yet another piece of strategy, which was, Don't have two subcritical things. Have two things that fit together, that complement one another. I think that has worked well. I think it has worked well because of Jonas, who is really extraordinarily good. And a Caltech undergrad [BS 1981]. Caltech is unusual in that it has hired back a number of its former undergrads. Sterl Phinney [professor of theoretical astrophysics], Ken Libbrecht, Jonas Zmuidzinas.

ASPATURIAN: Steve Koonin—

TOMBRELLO: Oh, my! I've forgotten about Steve Koonin.

ASPATURIAN: —Nate [Nathan S.] Lewis [Argyros Professor and professor of chemistry], Joe [Joseph Lynn] Kirschvink [Van Wingen Professor of Geobiology], Julia Kornfield [professor of chemical engineering]. The list goes on.

TOMBRELLO: It's nice that they go away. Though we have one great success who didn't go away, and that's Kerry Vahala [Jenkins Professor of Information Science and Technology and professor of applied physics]. Sterl Phinney, Libbrecht, Vahala were in the same class—the class of 1980 at Caltech. That was *some* class! That was an interesting bunch of people. We are lucky to have them back. Vahala also got his PhD here with Yariv. Libbrecht worked for Bob [Robert H.] Dicke at Princeton. Phinney went to Cambridge. It's nice that they get away and see a bit of the world and then realize that Caltech is Heaven. They come back. It's a great place, and our students are fantastic. But I've said that so many times, it's getting tiresome. You're beginning to see what I'm trying to say: There was a strategic framework for everything.

ASPATURIAN: Within which you were establishing a number of new initiatives.

TOMBRELLO: Right. The idea behind the JPL thing was to cement the relationship with JPL, but at the same time to complement some of the things on campus that we needed to complement. Didn't hurt JPL either. That's one of the worries for the future: Will Caltech always be able to hang on to the management contract for JPL? There have always been these threats from NASA about putting it up for bids. We've dodged that bullet several times. Can we continue to? JPL is very important for Caltech, because of the leverage that both sides get out of it. These joint appointments mean a great deal. We've done two small spacecraft projects within the PMA Division to help JPL out. One was GALEX [Galaxy Evolution Explorer], Chris [D. Christopher] Martin's [professor of physics] ultraviolet orbiter. Now we're doing NuSTAR on campus for Fiona Harrison, which is an X-ray measurement project.

The division's relationship with JPL has been good—in fact, I used to have regular meetings with Elachi, because he and I were opposite numbers for a while. When he moved up to the directorship, I met with Larry Simmons [deputy director of JPL's Space and Earth Science Programs Directorate]. When Simmons retired, it was Jakob van Zyl . It was very important to stay in step with someone with whom you had so many joint projects. It meant I had a lot of extra activities, but it also meant that I have an awful lot of opportunities with JPL. As I said, when we needed money to get the CCAT concept study started, half the money came from Jakob van Zyl and half of it came from me. I would say that since then, more money has come from JPL than has come from Caltech for CCAT, which would be a 25-meter sub-millimeter telescope located above 14,000 feet in the Atacama Desert in Chile. We're at a point in time where it finished quite well in the Decadal Survey [of astronomy and astrophysics]. That came at exactly the right time. I felt very good about that survey. Two of the projects I started at Caltech finished high up in that ranking—TMT and CCAT. I was very, very pleased. I keep hearing, "Well, you picked good ones." The answer was, "No, we made them good and therefore the Decadal Survey had to pick them." [Laughter]

ASPATURIAN: That's an interesting point of view.

TOMBRELLO: You had better try to work very hard to make things that people will have a hard time turning down.

ASPATURIAN: I wanted to ask you about the TMT vis-à-vis Carnegie's telescope project.

TOMBRELLO: That's an interesting story. It goes back to the previous director, before Wendy [Freedman, director of the Carnegie Observatories].

ASPATURIAN: Wendy being the current director.

TOMBRELLO: Yes. I have always gotten on very well with Wendy. The previous director was Gus Oemler. He wanted to be part of CELT, as the TMT was known then, but he didn't want to put up any money. We reluctantly told them no, that everybody had to contribute. You couldn't have partners who weren't contributing something. He could have figured out a way to contribute in kind. So this was one of the great losses, I think—that Gus didn't have a vision, and by the time Wendy, who does have a vision, came in [2003], it was too late. It was a lost opportunity. There were several tries to do things with Carnegie. I had hoped we could set up a joint institute in theoretical astrophysics. Wendy even found some money from Richard Meserve, head of Carnegie [Institution for Science], for that. But that has never really taken off, either. I didn't get much support on that from David Baltimore, because he had another idea about a joint project with Carnegie, in biology. That was because Maxine Singer, who headed Carnegie before Meserve, was a friend of his. That never worked either. No. Not bringing Carnegie on board was one of the failures. I tried a number of times, even after it fell apart with

Wendy, until well into 2007. Richard Ellis and I worked very hard to put Humpty Dumpty back together.

ASPATURIAN: What was the proposal?

TOMBRELLO: Basically, to figure out a way among ourselves to go forward with either one telescope or two telescopes, but with a coherent plan. Personalities get to be a controlling interest in these kinds of things. Some of the difficulties were relics of the old Caltech–Carnegie divorce in optical astronomy, which I mentioned in an earlier interview [Session 4]. Part of it was the history with Gus and the failed attempt at being part of TMT. Gus thought he could do it without putting up any money, and Caltech was putting up a substantial amount of money. The University of California, particularly Santa Cruz, was putting up money. It just wouldn't have worked to let somebody in for old times' sake, when there weren't any old times that were very positive. It was a lost opportunity, and now we're left with a competition between two major telescopes. I think TMT will win. I like the design better personally. I don't like the design of Carnegie's GMT [Giant Magellan Telescope]. But I do think there are some advantages, because they have a site. Carnegie actually bought the land in Chile in the days when they set up Las Campanas. It's a reasonably good site. There are some better. But still, it's an attractive offer to have all that land and a place to put this telescope. If Gus had said any of that and thrown that in as a way of getting this thing started, it might have flown.

ASPATURIAN: I have a question for you. If I recall the Decadal Survey, the NSF has been urged to make some kind of decision regarding TMT and GMT. I assume they will to some extent be guided by Congress, which surely would be influenced by the fact that the plan is to put the TMT in Hawaii, which will keep jobs in the United States. Do you see that as a potential asset?

TOMBRELLO: I believe I agree with what you said. The NSF will find it easier to pick a U.S. site. I think there's a lot of sentiment for putting it in Hawaii. Even before we had any serious money for TMT, we started the site survey, because we knew that was going to be the longest item in there. We did probably more stuff on site selection in Chile, and we looked at all sorts of other places, but not in detail. We studied prospective sites from satellite photographs and things like that, trying to get an idea of cloud cover and other things. How many good nights do you have? How much low-level turbulence in the atmosphere? You have lots of very complicated stuff to consider. So we ended up with three sites—two in Chile and one in Hawaii. Now I'm going to have to get into the political aspects of it. Hawaii is a political zoo. It's Mississippi with mangoes, I always say. You've got the native people, whatever they are; there is so much inbreeding it's almost like the Shinnecock Indians with their casino on Long Island.

ASPATURIAN: In the Hamptons. I was just reading about that in *The New Yorker*.

TOMBRELLO: Native Hawaiians make up about 5 percent of Hawaii's population—but that 5 percent represents a margin for a potential election victory, so it's hard to get a politician in Hawaii to completely disregard the Hawaiian groups. So far the Hawaiian groups are divided in many different ways. They range from people who are totally rational and are saying absolutely the correct thing, which is that we need more jobs and we need local industry, to the group that wants to bring back the king or queen. It's hard putting these groups together. It's hard dealing with them. One of the ways they try to deal with big projects, if I can be candid—I mean, I have a home there; I pay taxes there; I think I know a little bit about the Islands—is how much money, how big a bribe can they extract to do stuff.

For a while, we really were making very little headway with Hawaii. But at one of the last planning meetings I attended, which was roughly around Christmas 2007, I met a new member of the TMT board, whose name is Henry Yang, and he's chancellor at UC Santa Barbara. He is a quiet man. He does not stand out in a crowd. He speaks softly. But I'll tell you, Henry Yang, like Bob Sharp, is a genius at strategy. I'm up there and I know I'm going to have to leave for the meeting at the end and go to the hospital, where my daughter Kerstin has just had her pancreas out; she had pancreatic cancer; this had been a terrible fifteen-hour operation. With pancreatic cancer, you don't dare be optimistic, but one tried to be. At that meeting, Henry, being an absolute genius, came up with the key point, which I should have gotten myself. There is a saying in Hawaii: Everything runs downhill to Honolulu. And yet we'd been trying to deal with Honolulu when we were going to put a telescope on the Big Island.

Henry came up with the strategy of how do you divide them? How do you go after the local people, the local political types on the Big Island? The islands are counties. Kauai, where my home is, is a county. The Big Island is a county—albeit a pretty big and interesting one. I realized it was a workable strategy. You now had to implement it. Just because somebody has given you the key point doesn't mean it all happens. The site survey has turned out well. Was the Mauna Kea location the best site? That's a complicated question. At the longer wavelengths it was clearly going to be best, and if astronomy keeps moving to longer, far infrared, wavelengths, it's clearly going to be the best site. If we're stuck in the near infrared and visible, it's as good as what we have with the Kecks. Maybe we had better sites in Chile. But Gordon, I think, was happier with Hawaii. We seem to have solved, or are on the road to solving, all the local political problems with Hawaii. I don't know how big the incentives are that have been created for local cooperation, but I think the Moore Foundation is willing to pay them. But I think the key to the whole thing was Henry Yang.

ASPATURIAN: His intuition was that you deal directly with the Big Island rather than with Honolulu?

TOMBRELLO: That we divide 'em. [Laughter] We played to this division. Henry knew how to do it and did it. A quiet man but extremely sound. He and his wife wrote us the nicest letter in Kerstin's last days. I think at some point in his life he must have—they must have had to deal with some sadness of their own, because it was a letter that was very important to us. Anyway, I think we've probably said as much as we can about TMT. We've talked about CCAT.

ASPATURIAN: I was going to ask about personalities and politics within the PMA division. I mean, it's quite a division. You've got three different disciplines.

TOMBRELLO: Yes. I created this analogy early on, once I saw the IACC and the other divisions in operation. I said, "You know, Engineering and Applied Sciences—the division is Yugoslavia, with all the internal divisions, all the history that people weren't even part of but remember: 'You can't imagine what they did to my great, great, great grandmother back in 1403'—that kind of thing." All these old feuds, and EAS suffers from all of that. I said HSS [Humanities and Social Sciences] was Belgium—split between two groups that are never going to get along. You know, it was that kind of divide. Somebody said, "Well, what about your division?"

ASPATURIAN: Can I take a guess?

TOMBRELLO: Yes!

ASPATURIAN: The U.K.

TOMBRELLO: Close. Canada. We are not gregarious like GPS [Geological and Planetary Sciences], but we are collegial. We have a few equivalences of the French-Canadians; those are the astronomers—bombs in the mailbox and that kind of thing. But still, when push comes to shove, we may be dull, but we're collegial. We're not gregarious. But we do agree on priorities. That's why I started out with setting those three priorities, and the whole division got behind it. Even math; because math figured in some of this stuff—like the Fairchilds. They get one of the Fairchilds every year. Hey! It's a win for them, too. And some of the string theorists work with the mathematicians. We can, when it comes to it, agree on a few priorities, whereas Biology at Caltech has a very hard time with that. I don't know what I'd say the Biology Division was. I'd say "dysfunctional" is the word that comes to mind, and that's not a national group. How can such a small group be so divided, so contentious?

But anyway, people say, "What was it like, being chairman? What problems did you have with your colleagues?" I'd say that most of my problems were with the Caltech administration or with the administrative groups at Caltech. At the end of the day, people don't always agree in Physics, Math, and Astronomy, but they can get together.

Another thing I did—time now to talk about Mathematics. Mathematics was probably down below number ten nationally when I came in. They had hired some good people. They'd lost some good people. My philosophy for them was that you always go for *really* good people. And don't get disheartened if you don't get them. You make a lot of appointments, and you're always going to lose a few. Some are not going to come. You make them good offers, maybe just a step ahead of where they are professionally, and you just play that consistent strategy. Now we're number seven, and I think I had a lot to do with that. I think at the moment they are losing a few people. You have to get used to it. It's still the farm-team problem. But we have ratcheted ourselves up. We were stuck down in the rankings with Yale, and now we've left Yale in the dust. We are number seven, and they are probably still stuck around ten or eleven. We have blown past a number of places. But we still can lose the good people I had something to do with hiring. The department just has to get over the fact that you're not going to get everybody you try for. But you always want to try for the best, because if you get them you win, and if you don't, the rest of the world still takes you seriously. There was some mathematician from Stanford on a PMA visiting committee, and he says, "You guys seem to get to these good people first. You're making it really hard on the rest of us." I said, "I hope to. In fact, I hope to leave you guys behind, too." Well, Stanford's well ahead of us, but we could take Stanford. We might have trouble with the big battalions—Berkeley, Harvard, and MIT. But, you know, being number four would not be bad, and it's achievable. Again, you've got to be *insistent* that the people have to be superb, so that when you get them the rest of the world cries in pain and envy. Where you're unsuccessful, they say, "God, those guys are determined, aren't they?" We've got to keep doing that, and I am not convinced we are doing it with the same insistence, determination, stubbornness, whichever way you want to characterize it. The mathematicians love it. Everybody likes to win.

It's depressing when you find somebody who glows in the dark and you just don't get them. Or you do something *dumb*. When Kerstin was dying, I was negotiating with this absolutely brilliant young woman—Maryam Mirzakhani**.** I had her. The question was her husband, who was good but he didn't get a job in engineering here. Even at Stanford, where she ended up, they only gave him an adjunct professorship that was sort of jointly with IBM Almaden. After the fact, I realized I could have hired him. If I'd made a package deal for those two, I would have got her. I would have gotten a young Fields Medal winner. She hasn't won it yet, but she's going to. She's going to be the first woman to win the Fields Medal. I had the framework; I made her a fantastic offer. She wanted to come. I was distracted just enough by my daughter dying that I just missed one detail that I shouldn't have missed.

ASPATURIAN: It happens in all walks of life.

TOMBRELLO: Yes. I don't like making mistakes. That's one I still replay in my mind, roughly three years later. I could have had her, I could have had her. And her husband probably would have turned out to be quite good enough, too.

ASPATURIAN: That's too bad.

TOMBRELLO: Yes. But, you know, people do make mistakes. As perfect as I am. [Laughter]. That was one I just should not have lost. Hiring Elon Lindenstrauss was a long shot. Lindenstrauss was an interesting guy. I don't know where he is now [Princeton University—*ed.*]—he's won the Fields Medal, which I knew he was going to do. He was a very interesting guy. When we discussed him on the IACC, some perceptive person—maybe Elliot Meyerowitz [Beadle Professor of Biology], I can't remember—said, "Look, there's some missing years out of his CV." I said, "He's an Israeli." "Well, what does that have to do with it?" I said, "He was doing some secret project. And one could guess it was either in intelligence or nuclear weapons or something like that. He's got a two-year gap in his history. He was doing something interesting. But he's not going to talk about it, and he's not going to put it in his CV." The negotiations were fun—it was fun talking to him.

I remember another guy I was trying to hire. I was trying to steal him from Berkeley, as I stole [Thomas B.] Graber and Ooguri from Berkeley. It's harder to steal from Stanford. Guys like [Douglas] Osheroff there are really tough. It was hard to steal from Harvard, because of Larry Summers, who got personally involved in all these deals. Berkeley was dead easy. I remember talking to this candidate whose wife was in finance. I said, "L.A. is not a finance center. But, you know, I will even try my brother-in-law." "Who's your brother-in-law?" I said, "Robert Merton." He said, "Oh, I know who Robert Merton is. You don't have to explain." But he was impressed that I was willing to just go to any ends to find his wife a job. What does it take? It was like the Chiara Nappi thing with Witten. If I solve the problem of the family, I get the whole package. If I don't solve the problem, I probably get nothing. That was the case with Maryam. I blew it. I blew it because I missed the thought that I could have hired that guy. He was a good enough engineer to hire as an assistant professor. Wow! So it's always a package deal. You make somebody an offer they can't refuse, and they come. Or it's the wife or the kids. I remember a guy I lost at Harvard—a condensed matter theorist. He dithered and he dithered, and he couldn't decide. Talking to him, I said, "How does your family feel about this?" He says, "My little girl is three years old. Her opinion is in Southern California it's warm. There's lots of sun. There's Disneyland. Daddy, why aren't we going?" I said, "Put your daughter on the phone." [Laughter]

ASPATURIAN: But he did not come?

TOMBRELLO: He did not come because I didn't get his daughter on the phone. I could have sold her in a minute. It's a package deal. It's what the family thinks, too.

ASPATURIAN: How do you know who the best people are?

TOMBRELLO: You don't. You don't. But you've got the usual stuff. You've got what they've published. And the staffing committee is very sturdy. They read this material. So we have pretty good track records on people. We know how they've performed in various races. It's like buying a horse. Occasionally you get a *Seabiscuit*, which comes out of the blue, or a *Zenyatta*. Great race horses that were undervalued when they were young and people got a bargain.

ASPATURIAN: Do you have an example?

TOMBRELLO: Who was undervalued when he got here? A person people didn't take quite as seriously as they should have was Danny Calegari [Merkin Distinguished Professor of Mathematics]. We got him as an assistant professor [2002]. He changed his major in college. He'd been a poet, and a good one, from Australia, although I think he got his PhD in the United States [UC Berkeley, 2000]. He wasn't exactly undervalued, but more like *Zenyatta*—the price was reasonable. But considering the subsequent performance, clearly we got a bargain with Danny. In some ways, I knew he was going to be interesting, because even before he came I put him on the Math Advisory Committee, which does the staffing in math. He would call in with his comments, and he was very insightful about this. He was young. He was totally committed, and he had good taste in people. And I knew this guy has got something; and clearly he did—he got to be a professor, and a named professor, very quickly. He has won one of these Clay prizes. He wasn't exactly a sleeper. This was not *Seabiscuit*, but he might run like *Seabiscuit*. He was clearly a bargain.

Another hire that surprised everybody, although he wasn't exactly a bargain, was Alexei Borodin [Binder/Amgen Professor of Mathematics]. I hired him right out of grad school as a full professor [2003]. Boy, did that cause problems on the IACC! It sort of divided the vote. Very young. Publishing great mathematics as an undergrad, so even though he was young, he had a track record that went back. Stolper, who was GPS chairman at the time, was very much against it. Koonin was skeptical. So Koonin and Alexei and I met for a drink at the Athenaeum. At the end of it, Koonin just looked at me and nodded. Go for it, kid.

ASPATURIAN: That reminds me. Chris [Christopher M.] Hirata—is he still here?

TOMBRELLO: Oh, yes! He's back. Yes, he came as a fifteen-year-old undergrad.

ASPATURIAN: Do you want to say a few words about him?

TOMBRELLO: I think he's wonderful! Well, I hired him, of course, as an assistant professor [of astrophysics]. He's just been reappointed. I think he's very interesting.

ASPATURIAN: He's in theory, I believe.

TOMBRELLO: He's in theory. But he's capable of doing experiment. I think he can operate on the boundary line. He did a number of interesting things in both areas as an undergrad here. We hired him from Princeton three years ago. What has he done? Well, there are some people I tried to hire whom I was willing to put up huge amounts of money to get but didn't get, and now Chris has proved that some of their work is wrong. Somebody said, "What's he done?" Well, he's destroying the careers of several people who are much farther up the food chain than he appears to be. I think he's a winner. He doesn't have the flamboyant style or the charisma that a lot of people want, but he's awfully good. I would predict great things for Chris Hirata. I like him very, very much.

ASPATURIAN: How old is he now? All of twenty-three, maybe twenty-four?

TOMBRELLO: A little older than that, but not much older. He's still in his twenties, I'm sure.

ASPATURIAN: I remember when he was first admitted to Caltech. His mom used to walk the family dogs across the campus. A lovely woman.

TOMBRELLO: Yeah, he was so young he had to bring a parent with him and live off campus.

ASPATURIAN: Just a real *Wunderkind*.

TOMBRELLO: I just think—I hope—it's one of the great hires I made. Right now he is busy hanging crepe over other people's theories. He will have great theories of his own. Some people criticize the fact that he's mostly just proved that other stuff is wrong. I said, "Look whose original ideas he's proved wrong!"

ASPATURIAN: Such as?

TOMBRELLO: A cosmologist named Matias Zaldarriaga, who's now at the Institute for Advanced Study. I tried very hard to hire him. I thought I had him. He went back to Harvard—and this may be an apocryphal story, but I don't think so: He met with Larry Summers. I didn't think Larry Summers even knew what a young cosmologist was, but he said, "Kid, I can raise ten million for you." I lost him. I said to myself, "I've got to raise ten million," and that's when I went after Robinson and went after a couple of other Moore grants and got them. I thought, "I'm not going to lose anyone else because I've been outbid by Larry Summers."

ASPATURIAN: Lisa Randall was here for a while, a couple of years ago [Moore Scholar, summer 2008 and spring 2009].

TOMBRELLO: Interesting story. Yes, I initiated that, to try to hire Lisa.

ASPATURIAN: Just for the record, she's a theoretical physicist at Harvard.

TOMBRELLO: She's a professor at Harvard. She'd been any number of places for short periods of time. She's been at all the great schools as a full professor for a couple of years at a time. She's been at Harvard the longest. I think she's brilliant. This is going to get locked up for a while: She's very insecure. For someone so brilliant and so pretty, and who has had such presence, she is, I think, desperately insecure. It is hard to negotiate with somebody who is insecure, because you can never get them to agree to an offer. They seem to agree, and then they think, Maybe I can get more. That doesn't work. I got her here, kept her here; I like her very much. I think she has a real vision for the field of phenomenology and high-energy theory.

She's one of the great speakers. She'll be on campus the end of next week for [associate professor of physics] Maria Spiropulu's little event called "The Physics of the Universe." At that event last year, Lisa gave by far the best talk. I'm certain this year it will be at least as good. She thinks clearly and she speaks clearly. I like her a lot. But I failed in dealing with what I can only describe as insecurities, and [Andrew] Lange—we thought he might have better luck with her—ended up in the same place I was in. There was nothing you could do with it. They're bringing her back yet another time, I think, as a Moore Scholar. She doesn't want to teach. That causes a problem. Everybody in this division teaches. Feynman taught. Gell-Mann taught. All of us teach. You don't teach much, but you teach and you teach well. She has the makings of a very good teacher. She didn't want to teach, she wanted to be special. We lost a winner of the Veblen Prize in geometry to the Institute for Advanced Study because he wouldn't teach. That's just too bad, because at Caltech everybody teaches.

ASPATURIAN: Was that David Gabai?

TOMBRELLO: Yes, David's now at Princeton. We replaced David with Calegari. I like David very, very much, but he was the Karate Kid, very much involved in karate, and I kept telling him as he was leaving that Mr. Ohshima was *not* at Princeton. That's the farm-team problem, that you lose guys like David. But we got one of his first-born and that's OK, too. Graber and Calegari are as good as David Gabai, though I miss having David here. Losing people is something you have to get used to. As long as you can replace them with people who can hit the ball just as far, that's OK.

ASPATURIAN: How about the post–Tombrello era in the division? It's been two or three years now since you stepped down [2008].

TOMBRELLO: Well, I've had a hard time with the administration at Caltech.

[PORTION TEMPORARILY CLOSED, pages 220-221]

ASPATURIAN: How do you feel your successors have done in the division?

TOMBRELLO: Well, it's kind of unfair to make judgments. After me, [Andrew] Lange was in for a year. Lange probably should never have taken the job. He was a superb scientist; I nominated him for the Nobel Prize several times. I got him several prizes he wouldn't have gotten without active intercession. Because, you know, somebody's got to nominate you for a prize. You can't nominate yourself! I got him some nice prizes. Made sure he got an endowed chair before he asked for it. Got Thorne and Readhead to help me get him into the National Academy [of Sciences] before he worried about it. As division chair, between [Marc] Kamionkowski and Lange—that is, the theoretical and observational cosmology work—I raised probably about $23 million for two people. I got a Moore grant for Lange, out of turn. How did I do it? Jennings was recruiting speakers for one of the off-site trustee meetings—that one was probably at La

Quinta. I said, “Wow, I’d like to throw Lange’s name in.” Lange was here, but he and Frances were looking around. Somebody was after the two of them—perhaps Stanford. I wanted to do something. I’d raised money for the second flight of BOOMERanG. I’d raised money for the some of the equipment development. Not huge amounts of money—a few million here and there, but enough. I told Lange this was his shot, and that we could probably get about $12 million out of the Moore thing, out of turn, if he produced a performance at this gathering of suitable quality. Lange was a performer. They mobbed the stage; it was like something out of the Roxy Theater. The trustees were first cheering and then up there trying to talk to him. It was inspiring. It was beautifully done. Twelve million dollars out of turn. Not so shabby.

The interesting thing was the Keck Foundation. This shows low cunning, but also strategy. David Baltimore had managed to offend the Keck Foundation—or Bob [Robert A.] Day, who’s chairman of the board of the Keck Foundation, by pushing this proposal for KISS — the Keck Institute for Space Sciences. The foundation didn’t want it. Baltimore got the Caltech people associated with the Keck board to back it. Day felt that lobbying individual members of the Keck board was unfair, out of turn. He, by God, was not going to put up with it—he might let KISS through, but Caltech was a dead issue. So we angered the chairman of the Keck board. Baltimore did not have a delicate touch.

Now, I wanted to get a grant for Lange out of the Keck Foundation. The administration had just proposed these division chair’s councils—you know, outsiders who come in and give advice to the divisions and who will also serve as a kind of fan club to help you raise money. This was the summer of 2007, something like that. Some of the trustees, including Bill [William H.] Davidow and David Lee, have agreed to be on this thing for PMA. Another trustee, Charlie Charles R.] Trimble—Trimble Navigation, home GPSes, former Caltech undergrad—has agreed to be on it. Quite a charming man, a wonderful man. He won my heart when he compared me to David Packard. I thought to myself, well, I’m not David Packard—I knew David Packard—but it was a very nice thing to say. At the same time I got this mess on my hands at the Keck Foundation. The board is antagonistic, but now I have this chair’s council thing, and I meet a new member of the CARA [California Association for Research in Astronomy] board—the board that runs the Keck Telescopes—and they always have a representative from the Keck Foundation. It’s young T. J. Keck, the grandson of the Keck who gave us the money. I like T. J.; he’s charming; he’s sweet. He seems younger than he is—pretends to be dumber than he is.

He's not dumb. OK. Then Jim [James P.] Lower, who had been the executor of the John Robinson estate, calls me on the phone and says, "We've settled the Robinson estate. Are you now just going to say goodbye and forget about me? I've had so much fun at Caltech." I said, "Jim, you called at the perfect moment. I'd be honored if you'd serve on my chair's council." And he just also happens to be the general counsel for the Keck Foundation. I get hold of T. J. and I say, "T. J., I've just talked to Jim Lower. He's agreed to serve. Would you agree to serve?" I've now got the Keck's general counsel and a member of the Keck board. Do I ask T. J. to solve my problem with the Keck board? Of course not. But T. J. and Jim want to solve my problem with the Keck board. Of *course* they want to. I don't *have* to ask them; they're part of the team. T. J. got it through, with Jim's help, I'm sure, Jim being a clever strategist. Hey, to quote my grandfather, "God never told you to be stupid, Tommy." Put the right people on, sometimes they give money; sometimes they give something that's better than money. They give you enthusiasm. They sell your program for you.

That was a nice little grant for Caltech, and very nice grants for Andrew Lange. I'm sure it will still be well spent. I think Jonas Zmuidzinas is putting a lot of effort into keeping the Lange projects going. See, it's all part of this strategic vision. It isn't the way Development talks about it. "Go and ask the person for the money"—the *ask*. It's bullshit! Wasn't that way with Mr. Cahill. Wasn't that way with Walter Burke. Wasn't that way with the Keck Foundation. Wasn't that way with Gordon Moore. It was, "Can I give you a vision for what I'm trying to accomplish?" Through providing this understanding, we were able to get money for Kip Thorne and Marc Kamionkowski. But there's a certain piece to this that you can never completely predict—we were not able to get money for Roger Blandford. God is fickle, and donors are fickle. Somebody said, "What's it like dealing with donors?" I said, "Well, it's as if you had a Sears Roebuck catalog and you're taking somebody through it. You turn page after page after page, and finally they look at something that you didn't even want them to see and they say, 'You have that in pink? I might buy that.' That's what it's like." I said, "It's not frustrating, because after all, they want something. But they didn't want some of the stuff you wanted to sell them." But they wanted *so* much of what I wanted to sell. I'm *extremely* grateful to the people who were willing to accept the things I tried to make them part of.

Chameau does not like me to talk about a strategy, doesn't believe in strategy. I have to admit I didn't win his heart when I said, "Jean-Lou, that is just a dysfunctional strategy, to have no strategy." I'm not a sweet person; he has every reason to dislike me. But that's fine. What worries me is, I think people tend to mistrust strategy—or not understand what strategy is. They want it simple. The current PMA chair, Tom [B. Thomas] Soifer [professor of physics], is a very fine man. He's a fine scientist. He's a fine person. But I think he feels very uncomfortable about raising money. Why would he feel uncomfortable about raising money? Because he was running the Spitzer Science Center, which at its peak was probably spending about $150 million a year. Hey! Donors is donors. His donor happened to be called NASA; and you have to still be the performing seal for NASA. And to be perfectly honest, I was talking yesterday to a woman in the director's office at JPL. We were talking about NASA administrators, and she said, "Has there ever been a good one?" I said, "Not for a really long time, Nora." I mean, OK, you've got to sell things to somebody like Sean O'Keefe [NASA administrator 2001-05]? I mean, come on. You're talking funding with Gordon Moore, who's a giant, and then you've got to sell a space mission to Sean O'Keefe?

ASPATURIAN: Actually, Sean O'Keefe turned out to be better than what came after, I think.

TOMBRELLO: [Laughter] Yes.

ASPATURIAN: I think O'Keefe came in with a mandate to be hard-edged about JPL, but then he got seduced by planetary exploration, as many do.

TOMBRELLO: He became more enthusiastic. I think Charles Elachi had something to do with that. Charles is always a performer and always had a good program at JPL. I worry about JPL, but I worry most about this Mars Science Laboratory, which is a bet-the-lab-on-a-mission that's way over budget and way overtime.

ASPATURIAN: They've done so well with their Mars program in recent years. That would be a pity.

TOMBRELLO: That's one that got out of hand. They did well with the Mars program because they kept them relatively small. More robust than they expected.

ASPATURIAN: And incredibly smart, dedicated people.

TOMBRELLO: They had some great teams, and I believe what happened was they got off to a very bad start with the Mars Science Lab. And the first chief scientist of the Mars Science Laboratory [2005-07] was our very own Ed Stolper, who does not appreciate engineering and thinks you can buy it. I will tell you, in spacecraft, just like in high-energy physics and in cosmology, if you can buy it, you don't want it. You want to develop something that's new and better. I attribute a lot of the problems with the Mars Science Laboratory to its first chief scientist. I've told a lot of people that. That may not be fair, but it's what I've said. In fact, my history of the Mars Science Laboratory was weird, because I first learned about it when I was off looking at a project for Schlumberger in Norway. For lunch they just threw in something extra, which was their proposal to be part of that mission. They showed it to me, and I said, "God, that is the weirdest, shakiest mission I've ever seen! Is that something ESA [European Space Agency] came up with?" They started laughing. They said, "No. It's in your backyard. It's from JPL. What do you think is wrong with it?" I said, "Look, it's a very big, very expensive mission. And one of the things about big, expensive missions is you don't have single-string failure modes. You have so much redundancy, because you're paying for something that will work, no matter what, like Voyager. Now, Voyager had an absolute genius for a project scientist, Ed Stone. That is one of the best things that happened in the 20th century in science. Fantastic thing; and Ed gets a huge credit for that.

Let's talk about him for a minute. I've known Ed Stone since he came here as a research fellow in cosmic-ray physics [1964]. One thing that was clear from early on was that Ed was an expert at detail. He could take something that other people had done in cosmic-ray physics and just make it better. It was taste, hard work, insight, whatever. At that time, Ed was a youngster at Caltech, and he was just chewing up the great people in the field, because his experiments worked *that* much better than theirs did. This runs through all the things he did—the balloon stuff, subsequently the ACE [Advanced Composition Explorer] mission, the Voyager mission. Harold Brown thought that the mission that became Voyager was in trouble. He threw Ed Stone [then an associate professor of physics] in there [1972]. It was a brilliant pick. Ed was, I think, what made Voyager such an extreme success—one of the great scientific achievements. It wasn't without troubles; constant troubles. It was out there working far longer than anybody had intended. It was doing things that nobody had intended. Things went wrong, and they worked around them. It was a strong team, led by a brilliant scientist. I've already declared my absolute admiration for Ed Stone, so anything I say next is not meant to detract from that, but is meant to make him into a human being who had other characteristics too.

He became PMA division chair [1983-88] after Robbie Vogt. That was not easy for him. Robbie had been senior to him in the cosmic-ray group and probably had something to do with recruiting Ed onto the team. I think it always bothered Robbie that with Voyager Ed was now advancing beyond where Robbie had been. Now he was Ed's boss, as provost, and as I mentioned earlier about Carl Anderson and Bob Bacher, this was another case where the provost was still trying to be PMA chairman too. Stone managed to work around it. But one thing I've learned about Ed is that as a manager Ed always sails before the wind. Not in equipment design. Not in mission design. But in decisions in the division about whether to keep somebody or let them go, to give them tenure or not—Ed always took the easy way. He compromised. Was he a successful division chair? Yes, he was. But there was always this question when push came to shove: Would Ed take the easy way out? And he did, any number of times. But, as I said earlier [Session 2], he was also the one who gave me the chance to organize the physics staffing committee. He backed it. So am I going to complain about it? No, not really. But Ed is not perfect. He is *nearly* perfect. [Laughter]

Then he went to JPL as director [1991], and this taking the easy way is a bad characteristic to have as director. He got led astray by the head of NASA, Dan Goldin. Dan is an interesting story in his own right. In 1992, I was on the Space Policy Advisory Board, a large, powerful group. It had Edward Teller on it, and sometime toward the end of 1992 Edward managed to get to the person we reported to, [Vice President] Dan Quayle, and sell him this story that Admiral [Richard] Truly—who was the NASA administrator—was doing a bad job and should be replaced. This story came to me from Allan Bromley, who was presidential science advisor at that point to Bush 41 [President George H. W. Bush]. Truly was brought in to talk to the president. Bush has to explain that Truly is being fired. Truly says, "Mr. President, can you give me a reason?" Bush says, "Frankly, I can't." [Laughter] It was Lowell Wood and Edward Teller—Lowell was the evil little person who was a protégé of Teller's, and not one of his successful ones. Well, successful in some ways; he was one of the architects of Star Wars, a lot of it fraudulent, that Livermore did. So Goldin got in because Teller and Wood thought that they could control him. Well, the first thing anybody learns about Dan Goldin is that he is his own man. He may be strange; he may have strange ideas; but he is not stupid, and he is not going to be controlled by Edward Teller or Lowell Wood. So I'm sure the most disappointed people in the whole universe with Dan Goldin were Teller and Wood. They had picked this guy out of TRW, and they thought he would be their person, and he wasn't anybody's person except Dan Goldin's. Now, Dan became a fan of Total Quality Management [TQM]. Ed Stone tried to get the whole lab to buy into it and insisted on it. But there was one critic of TQM that everyone adored. His name was Charles Elachi, and he basically said to Ed Stone, "It's bullshit. I'm either going to do my job or do TQM. Ed, you choose." [Laughter] So fortunately, at least one person at JPL never bought into TQM, which was a pile of crap and drove the lab nuts. Elachi won the hearts and minds of the lab at that point.

ASPATURIAN: Why do you think Dr. Stone went for this?

[PORTION TEMPORARILY CLOSED, pages 227-231]

[RESUMES, REMAINDER OF PAGE 231]

ASPATURIAN: That reminds me, were you involved at all in choosing the architect and the design of the Cahill?

TOMBRELLO: Yes! Oh, you want to talk about the Cahill?

ASPATURIAN: Sure, since we were talking about architecture.

TOMBRELLO: We will not talk about the stuff on top of it, because I don't know anything about the deal on that, but I suspect Caltech has been fleeced on the solar cells. I may be wrong. We had a perfectly serviceable architect originally, because we had to have a kind of a preliminary design for the building. Baltimore didn't like it—said it looked like a bank. Well, it looked like a pretty fancy bank, but it did look like a bank. And David wanted a signature architect. I figured this was going to be trouble. Now, there were two signature architects that were being considered for buildings on campus. One was Rem Koolhaas. If you've ever seen the Seattle Public Library, you know this guy's a genius. But as it turned out, he was ill matched to Caltech, and we ended up basically having to write off the contract with him and get a new architect for the Annenberg Center [for Information Science and Technology].

ASPATURIAN: How was he ill matched?

TOMBRELLO: He didn't understand that there are budgets and that one actually has to hold to a budget. And one has to agree to a design and someday fix it. Well, I got Thom Mayne for the Cahill. I didn't know Thom Mayne. I looked up some of his buildings. Looked pretty good to me. But I thought, "I've got my deal. I'm going to be dealing with a prima donna. I don't know how this is going to come out," because I was watching the Koolhaas thing go on in the background, and "Oh my, I don't need this!" I meet Mayne. I fall in love. This guy is fantastic. I've got a big building over there, 100,000 square feet. I've got to keep it from feeling like you're in a hospital, and I can't waste space. We're in this meeting, and I say, "Those hallways are going to look like pipelines; they're three hundred feet long." He said, "I can solve that," and he starts slashing at a big drawing tablet and shows how he's going to break these hallways up in angles. He's going to break off the corners where they intersect and put things there like little coffee nooks. I think, This guy is solving a really hard problem that most architects would stumble over. He knows the building's got a fill factor that's unbelievably high. We packed a lot of stuff in that building, but his design doesn't feel like you're packed in there. He slashed through things. He's opened things to the sky. He's broken the corners. He's got these hallways that run on the diagonal. And he's packing people in there. It's genius. You sit down and you know you're working with a guy who just really loves what he does and he's *good* at it. But, you know, there's always this conflict between the budget and the artist. We're getting down to the late stages of the building, and I'm having to make compromises. And the compromises are all basically aesthetic: I'm not going to cut a single square inch off the inside of the building, but I am going to take the decoration off the outside. But we weren't fighting with one another. I said, "You know, I really sympathize. I *know*. I appreciate what you feel. I'm using your architectural touches on the outside as the bank account from which I'm funding this building." He says, "Yeah, you're just like all my other clients." And laughs. [Laughter] We would go off and get a drink afterward. He's a delightful person. He charged less than the plain-vanilla architects. He was a genius. He came up with innovative solutions. He can think. He can talk. He can inspire his people. I just had so much fun. Mr. Cahill hates the building—thinks it's ugly. I *love* it!

ASPATURIAN: Was that a problem?

TOMBRELLO: For me?

ASPATURIAN: Well, I mean, had the money already been signed over?

TOMBRELLO: Yeah, yeah, yeah. Mr. Cahill was unhappy, but then I meet this guy Richard Koshalek who now runs the Hirshhorn Museum, but he used to run Art Center [Pasadena Art Center College of Design]. I was at a meeting with some people from the Smithsonian—there was something I was doing with a little company I consult for. Koshalek walks up to me at this meeting, puts his arms around me, and says, "You have done a wonderful thing." I say, "You like the building?" He says, "I *love* it! It's like nothing else at Caltech." "Yeah!" I say, "Some people don't like that." He says, "You've done a wonderful thing." I said, "No, Thom Mayne has done a wonderful thing. But I agree with you. I love that building; I think it's great. I think it's unusual. It works." I read a book about buildings and how they evolve. A good building evolves; it doesn't stay the way it was. It grows in different and interesting ways as it gets older. I wanted to do a sort of post-completion, post-occupation, survey of the building—a survey to see where we are. What did we get right? What did we get wrong? Where might we go in the future? But no one's ever risen to that occasion. People don't do that. But I think it's sort of a lessons-learned thing that you do with projects often. What did we learn? What would we do differently? There's got to be more of that. We're really just talking about education, even if it's educating ourselves. That building was fun.

ASPATURIAN: That's a good note on which to end.

THOMAS A. TOMBRELLO

SESSION 9

December 31, 2010

ASPATURIAN: So we are at our final interview with Professor Tom Tombrello, and you are going to talk today about your graduate students.

TOMBRELLO: Well, some pieces of this we've already talked about. I got to Caltech. They had a new accelerator. The professors were not as closely involved with the students as the students might have liked and so I just fell into this. I was working with everybody's grad students, and we were publishing all kinds of things. I worked with Peter Parker, who's a professor at Yale. He was Ralph Kavanagh's student. I worked with some of Charlie Barnes's students, including an African American student named Lionel Senhouse. Lionel and I hit it off.

ASPATURIAN: What year are we talking about?

TOMBRELLO: 1961.

ASPATURIAN: OK, early days at Caltech.

TOMBRELLO: Early days. The students were the big part of my life. I wasn't advising them officially, but I was working with them, and we were having a marvelous time doing experiments together. I wasn't Lionel's thesis advisor, but basically some of the stuff we published really was just the two of us, because Barnes hadn't been involved in it. It was a curious thing: Southern boy and African American from the bottom of society in New York. His father had been a custodian at a subway stop; that is as far down as you can pretty well go. One day he said, "My father was a pearl diver." I said, "Yeah, I did some pearl diving myself." He said, "You know what I'm talking about?" I said, "Sure. It's cleaning toilets. I was a lifeguard one summer. At the end of the day, you were a pearl diver. You cleaned the restrooms. This is the unglamorous part about being a lifeguard." He says, "Yeah. So you really know what pearl diving is." We hit it off.

This is something that has happened a couple of times. In California I was surrounded by faculty members who were liberals. And I have to admit, I'm a social liberal and a conservative economically—not that I believe the Republicans these days are any more conservative about money than the Democrats are. I'm sort of none-of-the-above politically. I give my absentee ballot to my wife, and she gets to vote for liberals twice. Somebody at the Bohemian Grove once said to me, "You don't vote?" I said, "Look I live in a blue state. It doesn't matter what I do. I give it to my wife. It makes her happy." Somebody said, "You're a genius!" I said, "Yeah, I know." [Laughter] Lionel and I often would set out to shock people. There's a Southern sort of black tradition of talking trash, you know?

ASPATURIAN: Yes.

TOMBRELLO: So Lionel and I would talk trash and people would be, if not outraged, mystified.

ASPATURIAN: In public? Where?

TOMBRELLO: Small social event—small encounters. Sometimes one of the Caltech trustees, Shirley Malcolm, and I do this at Caltech events, because Shirley and I adore one another. She's a Southern girl, and I'm a Southern boy; and we can talk trash and they just *look* at us as if we'd come from Mars. I remember we were doing that at the opening of Broad Center [for the Biological Sciences], and they weren't outraged so much as mystified. Where did this strange pair of people come from?

ASPATURIAN: You want to give an example?

TOMBRELLO: No. I do not want to have it down on tape.

ASPATURIAN: All right. I'm not sure how it would transcribe anyway.

TOMBRELLO: It won't transcribe well. Shirley is one of Caltech's very interesting trustees. Lionel Senhouse was an interesting grad student to have, and we had a lot of fun together. He and Peter Parker and I used to do things. For example, we would be running all night down in the tandem laboratory [tandem Van de Graaff accelerator], and that's in the sub-basement of Sloan. Throop Hall still existed in those days, and if you went up to Sloan's third floor, there was a vending machine up on one of the landings, where it connected Throop to Kellogg. In the middle of the night, Lionel would say, "Hey, I think I'd like to go up and get a Stokely." Now that was a Stokely Carmichael—our definition of an ice cream bar that was chocolate inside with chocolate covering. Or I'd say, "I think I'd like a Martin Luther King." You know, that was vanilla ice cream with a chocolate covering."

ASPATURIAN: Not fair.

TOMBRELLO: We were not politically correct in any sense, and any professor who was down there that got subjected to Stokelys and Martins did not know what to make of it.

ASPATURIAN: So. What happened to the student? Where did he go?

TOMBRELLO: He ended up going to Bell Labs and has retired, I think. He did pretty well there. A lot of my students went to Bell Labs in those days.

ASPATURIAN: Quite a story.

TOMBRELLO: So anyway, I go to Yale, miss the place, come back. We've talked about that. Picked up Andy Bacher, Bob Bacher's son, who had been Willy's student. He and I got on very well—he's still one of my very best friends. He's an emeritus professor now at Indiana University. I see him a couple of times a year, because he still owns his parents' Santa Barbara house. I picked up a guy named Bob [Robert J.] Spiger, who approached Willy, and got given to me. Bob was a very unusual student. Very large physique. Builder of boats. Extraordinarily good student.

I had a number of students all doing nuclear spectroscopy, some of it with application to nuclear astrophysics. So it fit into Willy's game plan. From somewhere in the early 1960s until probably about the early 1990s, I had something like thirty-five PhDs who worked with me. There were a lot of other students who were not counted as my graduate students, since they worked for other people, but in actual fact their work was influenced a great deal by what I was doing.

I always had a very good time at Caltech. For many, many years, it was idyllic, in the sense that Tommy Lauritsen and Willy Fowler got the money and I spent it and did research with it. It was only by the time we got into the late sixties, early seventies, that things got tighter, and that's when I started doing a certain fraction of my research in what you would call applied physics or applied nuclear physics—techniques from nuclear physics adapted to materials analysis, radiation damage, analyzing lunar samples. We talked a little bit about that [Session 2].

ASPATURIAN: Well, all of those are very interesting areas of investigation.

TOMBRELLO: It was slightly threatening to the people in Kellogg, because they did not want Kellogg to change and Kellogg had to change. As it turned out—if we jump ahead—it really did change after Willy retired and Koonin took the lab off in yet another direction. I think—I like to take the credit partly—that Koonin dared to change things because he'd been influenced by me.

ASPATURIAN: Jumping back for a moment to when you were doing these applied studies that kind of went against Kellogg's traditional culture, did that result in a certain amount of friction?

TOMBRELLO: Friction that I have to admit I probably ignored until it was too late to ignore—when they basically tossed me out as PI. We went through the story of how I got to be PI, and I think at first that a lot of people didn't like that. A lot of people did not like what I was doing because I would go off into things

For example, in the seventies the Chinese seemed to be making progress with earthquake prediction, which would be a big deal in Southern California. And so I got into it. Developed some new instrumentation, working with a guy named Mark Shapiro, who is a professor down at Cal State Fullerton. We came up with some very clever ideas, basically robotic instrumentation, which was totally new to geology in those days. We got the idea that we could put stuff out in the boonies if it could be kept safe. We put small robots that took radon data near ranger stations and things like that. It was done very cheaply. They grew to where they could take all kinds of data about things like gases dissolved in groundwater. In the mid-1970s, Intel and Motorola had brought out the first pretty-high-performance microprocessors. We took a look at them and picked Motorola's 6800 chip. I think we made the right choice. It was a four-bit microprocessor. It had minimal memory, but it was a computer. You could run a robot with it. So we put these things out there. They ran on batteries, but we continuously charged the batteries from the power lines. And because people tend to break into things or shoot them—we had a few cases of that—we bought little Sears utility sheds and sited them near forest ranger stations. We put the instrumentation into army-surplus fiberglass boxes and bought them a phone number. It cost $7 a month to communicate with them, so it was a step forward in gathering data in an efficient way. Remember, this is now over a third of a century ago. It was efficient. It was cheap. In 1979, I went to China and talked to the state seismological people about this project. They said, "How many people work on this?" I said, "We have one full-time equivalent." They said, "In China we have the human sea." Well, as it turned out, people were cheap. They would site instruments out in remote locations and have somebody living in a hut next to it. Alan Rice, who is now the division administrator in PMA, was one of the people we had who really, *really* was roughly our full-time equivalent for a while there. We had a network of something like a dozen of these things. That was one of the projects that some of the people in Kellogg truly hated.

ASPATURIAN: It sounds like you were taking, or trying to take, the lab in a more interdisciplinary direction.

TOMBRELLO: I was trying to explore the boundaries to see if there were things that would catch people's imagination and would not just be applied but might also eventually lead to some new science. Were they successful? Yes, but most of the successful things—particularly in the materials science and analysis stuff—occurred after I left Kellogg and was on my own. I think I've told that story. The seismic radon project died for an interesting reason. I had friends who backed it. Frank Press liked the idea very much. He was a true believer. We were all true believers, until we took enough data to realize that most of the signal was just noise. We thought we saw signals of precursors, but in reality the signal was just not something that stood out the way the Chinese claimed it did. We discovered that most of the Chinese data, and virtually all of ours, was related to geochemical signals that had to do with aquifer mixing, changes in temperature, and other phenomena that depended on very small changes in atmospheric pressure.

If you start doing carefully controlled experiments, you begin to realize the difference between noise and data. In this case, although the signal was very small, we're pretty sure we saw it. Our hope was that if you had this network out there, along fault lines where nothing had moved for a while but which had a history of seismic activity, you could localize areas that were much more likely to have an earthquake. What you wanted to do was have enough instruments out in the field so that when this fault broke, you would have a history of events and signals leading up to it that basically could be interpreted as precursors. If you saw those same signals at other places and no earthquake had appeared, you would know that maybe there was another cause, and so on. The real chagrin we had was that the program got terminated because they moved almost all the earthquake-prediction research stuff to Parkfield. But it was the same philosophy: Here is something that breaks on a regular basis, and we're going to take all this data leading up to the next time it breaks.

ASPATURIAN: They pretty much instrumented it up the wazoo.

TOMBRELLO: When they sort of terminated our program, I remember saying, "I'm going to be the witch at the christening. I'm going to bring down a curse upon you. I'm going to say to you, 'You're going to go up to Parkfield and you're going to sit there looking at nothing happening for years.'" And that's exactly what happened. There are probably a few people around that think I really did curse the Parkfield program, because the fault has just sat there for a very long time.

ASPATURIAN: It seemed like a good idea.

TOMBRELLO: It seemed like a brilliant idea—and it was the same idea as ours, but to put all your eggs in that one basket struck me as being stupid. With our program, we were covering 10,000 square miles. Now, granted, not very densely. We were hoping to deepen our coverage. But we were covering a lot of areas where there was a much higher probability of something happening, and the interesting thing about it is that we knew that something was going to happen down around Whittier Narrows. And maybe a year after they closed our network, they had that magnitude 5.9 earthquake that occurred, I believe, in the fall [October] of 1987.

ASPATURIAN: How did you know something was going to happen at Whittier Narrows?

TOMBRELLO: We didn't. We knew that there was a block there that hadn't moved, while everything else along the fault had moved. We were playing the odds. We didn't just pick *one* area. We picked as many as we could cover out in the field with these dozen robots. But, as I say, I don't know what would have happened— If you don't have the data, you can't say we would have seen a precursor. Anyway, it never happened, because the instruments were all closed down. I left Kellogg in about '82, and in '85 the program was shut down. It was a lost opportunity, one where if we had still been there in 1987 we would have known whether or not there were precursors for that earthquake. It was really too bad that within two years we could have potentially answered that question.

ASPATURIAN: You mentioned China. You've had quite an extended relationship off and on with various aspects of Chinese society—government, academia. Do you want to talk about that?

TOMBRELLO: Oh, sure.

ASPATURIAN: Particularly in the context of China's emerging now as a real superpower in a number of these areas.

TOMBRELLO: My first trip to China was on a delegation in 1979 in nuclear physics. Allan Bromley was leading it. He later became Bush 41's science advisor. I'd worked for Allan at Yale, and we'd stayed friends. Most of the people on the delegation were orthodox nuclear physicists. They were much desired by the Chinese, who wanted to hear their talks. I was mostly giving applied physics talks. The only people who wanted to talk to me were people who'd read my papers on accelerator design and wanted to pin me to the wall about details of how these particular resonating structures worked and where did I think the field was going. I didn't belong in this delegation as far as the Chinese were concerned. So it's very interesting that three years later, in 1982, my wife and youngest daughter, Kerstin, and I went to China for an international conference on earthquake prediction. Kerry Sieh [professor of geology] and his wife had been living over there—that was before Kerry came out of the closet. Suddenly the Chinese were eager to have me come talk at various places around China.

ASPATURIAN: What did they want you to talk about?

TOMBRELLO: Applied nuclear physics. Suddenly they had discovered, Hey, the government wants this nuclear physics stuff to have a payoff. Oh, we were popular! At one place close to Beijing, we were talking about analyzing materials, and they said, “Do you know what major important trace elements there are in beer?” I said, “No.” They were clearly in the pocket of Tsingtao Beer. They wanted to tell me about all their analysis of the beer and about how their research was really pinning down the good and bad things in it. It was such a total reversal of what had happened in 1979. By 1982 it was so clear they just wanted to talk about how you use science to do things that conceivably made money.

We had another interesting experience. We’d been out in west China at Lanzhou, visiting a research institute out there. I still get their quarterly, or yearly, progress reports. In fact, you see, a lot of the work at Lanzhou grew out of the fact that they have a big dam, a big hydroelectric project, out there. And though I never saw any of it, it was a bit like the Tennessee Valley Authority. You needed a lot of electricity to do isotope separation. Out there, as nearly as I can tell, that’s where they were doing U-235 production. I remember joking with one of the lab heads about it, and he said, “Yeah, you work on that kind of stuff, too. We can’t talk about it, but we have worked on similar things.” I remember flying in there. We’re on some small jet, probably some ancient Russian jet, ill maintained. We hit the ground and the plane rolls forever. Everyone says, “What’s going on?” I said, “I think they fly jet interceptors out of here. This reminds me of landing at one of those dual-use airports, like Albuquerque, that they fly jet fighters out of.” So as we are taxiing toward the terminal, you can sort of just see, in the early evening dusk, planes hidden behind camouflage nets. I say, “Yeah, I think it’s pretty clear, that’s what we’re seeing.” They still had radar installations on the tops of the hills. They still had an airport with jet interceptors. They were presumably set up to protect the isotope-separation facility. We never saw any of that. We just saw the university and this research institution.

There’s one more, small story about that visit. China was chaotic. In 1982 they lost our airline reservations for flying from Lanzhou to Shanghai, so they put us on a train with a guide. Soft seats. There’s quite a difference between hard seats and soft seats in China. And so we go across China on the train, which was an adventure in its own right—kind of fun. We stopped in Xian. So we get to Shanghai very early in the morning. We’re out in the old French section of town, staying in a very old, what probably had been an extremely elegant hotel, the Jin Jiang. It's got very high ceilings and huge, heavy long drapes. Stephanie and I were sleeping in, and late in the morning I get up, push the curtains aside, and look out. I say, "Stephanie, I'm having a hallucination." She said, "What's going on?" I said, "We're in the heart of the Red Chinese Empire. And I'm looking out at the flagpole, and it's got a Union Jack at the top of it." And Kerstin, who's twelve years old going on thirty-five, says, "Well, if you'd been up and about, you would have met her, too." I said, "Tiny twit, what are you talking about? Met whom?" And she said, "Maggie, of course. I've been downstairs, and I met her." I don't know what she did when she met Maggie Thatcher [U.K. Prime Minister Margaret Thatcher], but since she was born in Cambridge, England, she probably curtsied and told her she was a countryman. I don't know. Kerstin had just come off of being the only child at a big banquet at the Great Hall of the People. God, were there pictures taken of her, because she was unique! You know, you drive up and you're in a limousine and you get out and the press are taking your pictures; and out of this car jumps a twelve-year-old. Jumps out? Oh, no! She comes out, ultimate sophistication. You know, if you'd had a twelve-year-old boy, you would have had to lock him in his room. But a twelve-year-old girl! This was just perfect. So when I hear about Margaret Thatcher, suddenly I do a calculation. It's 1982. The lease for Hong Kong is up in 1997. Years later, I check this out with some friends of mine at the British consulate, one of whom was with Maggie on that trip. There's nothing in the newspaper, but clearly everyone in China knows what's going on. They've started the negotiations for Hong Kong. In 1997, I was at a conference in Japan, and suddenly we get this invitation, "Come over to China; we'd love to have you guys for a week. We'll pay for all of it." We said, "What is this about?" And when we get there, we're there just in time to watch the handing over of Hong Kong on television. They wanted us there to gloat. Hey, bragging rights—no problem. They got it back. But we'd been there for what was sort of the first step, totally inadvertently.

ASPATURIAN: You've been to China quite a bit. What do you think? Is China going to absorb Hong Kong or is Hong Kong going to absorb China?

TOMBRELLO: I think the Chinese have a very good game plan. You could see the beginnings of it in 1979, in the kind of trade goods they were expecting to sell and in the way the people were adapting to free enterprise. In ’79, as we were chauffeured about town, we noticed clusters of people with bicycles and boxes of what looked to be vegetables on street corners, where streetcar lines crossed. I said to the guide, “What’s that?” He started laughing and said, “You’re seeing the beginnings of free enterprise.” These people were coming in from the countryside with vegetables and selling them on the street corners. He says, “It’s not part of the collective. It’s a totally new thing.” So we saw the beginnings. It was matched to, I think, the nature of the Chinese people, who are clearly entrepreneurial. I think Deng Xiaoping was an absolute genius—he picked *that* avenue to begin opening the economy, and it worked. You could see it starting to work. I have a diary from that trip, and in it I wrote that the standard of living in China would stay low for a long time but that something was happening. There’s this old statement from Napoleon that China is sleeping, but it is a sleeping giant and when it wakes it will shake the world. Clearly, China was waking up. The question is, How long will it take? How fast will this accelerate? But even then it was very clear that China was going to be a model. I remember saying that China is a model for a lot of economic development around the world, in places where they’re going to have to start as low on the development scale as China did. I don’t know what the average income per person was in 1979 in China—maybe $300 a year? It’s probably ten times that now; I think it’s at least $3,000 a year.

ASPATURIAN: They’re aiming for $30,000. I read that somewhere recently.

TOMBRELLO: That’s going to be interesting. I’d say the real concern about whether that’s possible or not is whether there are enough natural resources anywhere to get another factor of tenfold for China.

ASPATURIAN: For that many people, true. And then you’ve got India next door, too, on top of that.

TOMBRELLO: India will take longer, though.

ASPATURIAN: Well, they have chosen a different path.

TOMBRELLO: Somebody very clever twenty years ago got India moving in that direction. Both countries are going to find obstacles, partly bureaucratic. In the case of China, there will be particularly a certain degree of, oh, I won't say criminal activity, but probably close to it, in the sense of entrepreneurship that crosses the line. It's a little hard to imagine starting a business in China without having a very powerful Chinese partner. I don't know about India, but probably it's easier there, provided you can get through the impenetrable bureaucracy. There's an enormous amount of poverty in India, though. China has been much more successful at cutting down on infant death and all of that. The infrastructure may be primitive in many places out in the boondocks in China, but there *is* infrastructure out there. There are roads. There are power lines. India is a very poor country when you get out of the cities. Very, very poor. And it will take a long time to do anything about that. I'm no expert on India. I'm certainly no expert on China, either.

ASPATURIAN: No, but it's interesting to hear your perspective. You've been to China a number of times, at different periods in its recent history.

TOMBRELLO: Well, I think when you interviewed me about the trip in '85, I may have told you the story: I got this call on the phone from somebody at the World Bank, asking me if I would go—such a great thing, to go see China. I said, "I've been. It's like living in a Midwestern YMCA." He said, "But you haven't hung up." I said, "No. I am totally, totally entranced by the idea of seeing something that big move that fast." Every time we went there, it was different. Our last visit, in '97, was to Shanghai. You know, what they say in China is, "Shanghai—that's where they do things first." That is not a compliment. I said, "Oh, it's just like L.A.!" [Laughter] But you know if there's a trend, like it or not, it probably started in L.A. and eventually spread to the rest of the country. Shanghai is actually very much like New York City. The Chinese say it's a foreign city in China. You know, it's a Western city, and I think that's certainly an attitude thing. It's full of Chinese people, but there is a New York attitude about the way they know they're special. They're not a big village like Beijing—which they often say there—they're different. It's fun going to Shanghai.

ASPATURIAN: 1997 was your most recent visit?

TOMBRELLO: Yes. There's no obvious reason to go back. Given the choice, I would go to Paris any day. Better food; better walking around—though I gather the subways are now improved. We got a Christmas card from somebody in Shanghai saying that they've extended the subway network. Hey! It's getting to be a very efficient city.

ASPATURIAN: Anything else you wanted to talk about? We mentioned a couple of people you thought you might want to discuss a little.

TOMBRELLO: Oh, you want me to talk about the story of Fiona Harrison.

ASPATURIAN: Fiona. You also mentioned Anneila Sargent.

TOMBRELLO: OK. Let's start with Fiona, because that's an interesting story. It starts before I'm PMA chairman, about 1995, when I'm running the staffing committee. There was some sort of—I can't remember exactly what it was, but it was a set of presentations on some new projects in 114 East Bridge. One of them was something called AMANDA [Antarctic Muon and Neutrino Detector Array], which later [2005] become part of the IceCube Neutrino Observatory [AMANDA/ IceCube was decommissioned in 2009.—*ed*.]. It used the Antarctic ice as a detector for high-energy particles, particularly neutrinos. You drill holes into the ice deep enough for the pressure from the overburden to squeeze out the bubbles. You put photomultipliers down there and run cables out. And you see the Cerenkov radiation from the neutrinos acting on and interacting with the ice. So you have one of the world's biggest neutrino detectors, done cheaply. I can't remember the name of the postdoc at Berkeley who came up with the idea. But Barclay Kamb was involved—he was down there studying the motion of the Antarctic glaciers. He used hot water to drill through the ice—which is thousands of feet thick—down to the basal plane, where it interacts with the surface. He's approached by this postdoc, who says, "What do you do with the holes when you're through with them?" Barclay says, "We pour water into them and freeze them up." The guy says, "What if I put something down in the hole." Barclay says, "You don't want to get it back? Fine with me." So that was the beginning. That was AMANDA.

So anyway, this was early in the game, and I am sitting in East Bridge next to a friend of mine named Buford Price, who is a professor at Berkeley. I was still riding high, because two years before, I'd stolen Andy Lange from Berkeley. I said to Buford, "Got him away from you." He says, "Yeah. But I got something better." I said, "What's that?" He says, "I'm going to hire Fiona." I don't know who Fiona is! But I pretend. I said, "Oh, God. You're a genius, Buford. You're the smartest thing alive. I mean, you're taking me, you know. You're going to get Fiona, and I don't even have a chance." And I'm thinking, I have to find out who the hell Fiona is. We take a break and I come back up here to check something, and I look in my mailbox and there's a note from one Fiona Harrison! Oh. And she's a named postdoc here. She's beginning to explore the possibility of applying for a faculty position. So I thought to myself, Oh, Buford, Oh, Buford. I go back and I don't mention this, and I said, "Buford, you're a genius. You got Fiona, and I don't have a chance." I thought to myself, she's mine. [Laughter] So, first step was, I meet Fiona Harrison. I realize that she is clearly one of the great experimenters. I mean, this division is full of extraordinary people who can do experiments: Jeff Kimble, Andy Lange, Jonas Zmuidzinas, Tom Phillips, Ken Libbrecht. They're just naturals. The list goes on and on. You put them in a lab, they can make something work and make something new. I realize, here's another one.

ASPATURIAN: And what was she working on?

TOMBRELLO: She was working with Tom Prince, and they were doing X-ray astronomy. I check with Tom, and he says, "Oh, she's good. Do you think we can hire her?" I said, "Oh"—to misuse a phrase from George Tenet—"a slam-dunk!'" I don't know if I used those words or not, but I just thought this was a shoo-in. I was as proud of having captured Fiona as I was of capturing Andrew Lange. She is extraordinary. I've always been pleased with how well she does what she does. I've always been extremely pleased that she's tough-minded about it. Now she's got this spacecraft project, this NuSTAR, which has a terrible history as far as she's concerned, because a lot of it coincided with the fact that her first baby, Erica, was dying. She lived for about eighteen months but was born with a terminal illness and was severely compromised. Fortunately, Joanna, who was born after Erica died, even looks a bit like Erica.

ASPATURIAN: She was a beautiful little girl. Red hair, beautiful little face.

TOMBRELLO: So Fiona was going through that, and she was going through the rejections of NuSTAR, some of which were handled quite badly by NASA. But it's turned out well. She's gotten the mission and it should fly in 2012.

ASPATURIAN: And NuSTAR is an X-ray space mission?

TOMBRELLO: Yes. It's a SMEX [Small Explorer] mission, which will launch, I suppose, from a Pegasus. Everything looks good so far. Fiona's got a good project manager. She's quite fantastic. So this is one of my coups. I love Buford Price, but it was fun taking Fiona away from him after he just assumed he had that one in the bag. She'd been a grad student up there. They knew her well before I knew her, but it didn't matter. [Laughter]

So, Anneila. Well, I've known Anneila forever. As it turned out, when I met Stephanie, they had been friends, so that we got the Sargents from both sides. She was a graduate student here. Then she was a postdoc. I think they didn't know what to do with her. They shifted her to the staff.

ASPATURIAN: Yes, that's right. She was a member of the professional staff.

TOMBRELLO: Then when they needed somebody to run Owens Valley [Radio Observatory], they put her back on research faculty. I think she's been systematically undervalued by her astronomy colleagues. She could have been provost when Stolper was chosen if she hadn't been trashed by some of her colleagues. I will not mention their names. I certainly don't believe they have an accurate assessment of many of her talents.

ASPATURIAN: She has done some very interesting work.

TOMBRELLO: She did some very nice work on those planetary disks. She's been a wonderful manager, and the people who work for her adore her. She really does so many things so well. She was director of OVRO. She was then director of CARMA. She was the first director of CARMA in the critical stage of finding a site, which was not easy. There was negotiating with the Forest Service. There was negotiating with the Indian tribes. There were all sorts of obstacles. She just kept at it. Then, of course, getting the thing up there and working—she was as good as you can imagine. While I was division chair, I was running a little training program for people to take on leadership positions. Anneila was clearly already a leader, and I just wanted to see if I could continue that education in a positive way. But she was already moving along. Andy Lange was one of the trainees. I put him on the LIGO oversight committee here with Emlyn Hughes [visiting associate in physics], whom I hired in 1995. Because Berkeley got so divided on whether to take Fiona or Emlyn Hughes, they lost both to Caltech. They sometimes shoot themselves not just in one foot.

ASPATURIAN: Do you think maybe it's harder when you're a state university—you're operating under more constraints?

TOMBRELLO: In that particular case, it was just the sociology of the department. But Emlyn, I knew, was also a promising person. So I put him on the LIGO oversight committee. He chaired it and actually, in my opinion, has probably been its most effective chair. He led them when [LIGO director Barry] Barish decided to retire early. [Kip] Thorne and Barish had agreed on Jay Marx [senior research associate in physics] as Barish's successor, but they had not systematically looked at some women, and I had to do a lot of very careful recovery from that to make sure that women actually did get included before a final decision got made. It looked like a setup deal, how they got a great director. But they really had to do the right thing. Because there were two women who wanted to be considered and who were clearly very qualified. So there were decisions that Emlyn and I had to make. They were not extremely popular with the LIGO people, but at the same time we ended up hiring their choice. Now that Jay's retiring, they're going to have to do it again, without having Emlyn running the committee. He did a great job. He left Caltech a number of years ago [2008] to go to Columbia. He's a professor there in high-energy physics and is part of the ATLAS detector group at CERN [European Organization for Nuclear Research]. I think he's a natural leader and will go far.

So I had trainees. Anneila was a very successful one. I didn't think she was going to be nanny to the undergrads [vice president for student affairs], but as near as I can tell, she's doing a good job. And it can't be totally easy. Well, it's easy in one respect—she's replacing Margo Marshak, and anybody after Margo or John Hall is going to look good. Not that John was as bad as Margo, but he didn't have the right personality to be the VP. Anneila does. It's working very well. I don't know whether she has had problems with the current administration or not, but I do know that she's had a trying time. That suicide cluster we had last year cannot have been easy—there were something like three students, plus Andy Lange. And for a school the size of Caltech, four people in a suicide cluster is a lot. The trouble with suicide clusters is you don't know what causes them, and when they go away, you don't know if *you* caused them to go away or they just have disappeared below the surface. They're very frustrating. I'm sure Caltech was frustrated. Cornell was having a suicide cluster at the same time. NYU went through this, too. At Cornell, they were jumping into the gorges. At NYU, they had this new library with an atrium and people were either jumping out of buildings or into the atrium. At Caltech, I think with one exception, they were using the so-called getaway bags, where you modify plastic dress bags and basically drown yourself in helium that you buy at some toy store, you know, for balloons. I think three of the four were like that. It's amazing how these things take on the characteristics of an infectious epidemic. I studied that a little bit last year, because I was so concerned when Andrew Lange died [January 2010]. Andrew, apparently, although none of us knew it, had had this tendency toward depression most of his life. If I had known, I certainly would have tried to discourage him from being division chair—not that I think that caused it. I think it was just one more thing in his life. I learned enough to know that suicide is a very complex issue. These university suicide clusters have got to be enormously frustrating to the people involved. I think Caltech made some mistakes. In retrospect, you can always see that mistakes have been made.

ASPATURIAN: Yes, hindsight is always twenty-twenty.

TOMBRELLO: Other leaders: There's Jonas Zmuidzinas, who runs the Microdevices Lab at JPL and has a joint appointment on campus; we talked about that. I tried very hard to develop a next generation of leaders at Caltech the same way I tried at Schlumberger. It's always been fun, watching the careers of these people at Schlumberger as they move up the food chain or move into something different. At Caltech, there were some I put into positions where they might develop as administrative leaders, because I thought they would succeed. Caltech's about doing good science and teaching, and it's just this extra bit if one or more of them turn out to also be able to run things. It should be no disgrace to "fail"—put that in quotation marks—at being an administrator. But it's very important to groom future leaders. Institutions do it well or badly.

Los Alamos is a wonderful scientific and engineering laboratory, but it is not structured to develop leaders, and in fact, trying to move from science to administration is discouraged even at the lowest level. So Los Alamos does not tend to develop leaders. At Livermore, on the other hand, young people are encouraged to move from being a classic scientist or engineer to a position where they are running a group of ten people. They get a lot of help from their colleagues and from the institution. And so it is no great surprise to me that Mike [Michael R.] Anastasio, who did all his professional development at Livermore, is now director of Los Alamos. I think it's perfectly natural that some institutions cultivate leaders and some don't, even though the two institutions can be equally good at doing science and engineering. Bell Labs, of course, has a wonderful history of developing leaders. I think IBM did as well. Some places probably don't do well at it. Developing leaders get killed off at some stage. It could be infighting; it could be the fact that the norms of the institution are such that they look down on people who want to move into administration. I think that's a little bit the view at Los Alamos—you've taken on administration, and therefore you can't be good at science. Well.

ASPATURIAN: There does seem to be a stigma that sometimes surfaces in connection with this in academia.

TOMBRELLO: A few people have to develop into leaders. Let me talk about another woman, because she's an interesting case: France Córdova. France was a technician in the [Gordon] Garmire astrophysics group. Somewhere in the early to mid-seventies, she came to see me and said, "I'd like to be a graduate student." I said, "Tell me about yourself."

She said, "Well, I have an English degree from Stanford." I think she taught in public school for a while. And then she got this technician job. She really liked science, and she decided she wanted to be a grad student. I said, "Well, France, you certainly have the ambition. Let's see if you have the discipline to do what I suggest. Go around to three professors who are teaching the required, first-year grad student courses in physics, and ask them if you can audit the courses and if they will keep track of your grades on homework and exams. If you do that successfully for even half a year, you will have three people to write letters for you saying basically that you are capable of doing graduate work at Caltech." She did have the discipline to do it, and she got into graduate school here, got her PhD with Garmire, and went to Los Alamos.

Apparently did very well there. By then, Gordon Garmire was at Penn State University, in astronomy. They were looking for a chair. It was a tiny little department, and they hired her as chair. She went from there to being chief scientist at NASA and from there to being vice chancellor for research at UC Santa Barbara, Santa Barbara being one of those schools that had gone from, you know, a beach college to something more in physics and materials science and engineering. I think our friend [Robert A.] Huttenback, who was not considered to be great by the people in HSS here, did a fantastic job there, carrying UCSB into the future. And so France then went from that to being chancellor at UC Riverside, and now she is president of Purdue University. She's an interesting protégée. We occasionally talk. I like her very much. I think she showed not only intellect but also the discipline to keep this moving forward in a direction she wanted to go. She knew fairly early on—I think probably by the time she got to Penn State as astronomy chair—that her future career was going to be linked strongly to running things. I'm picking a little bit on the women because I think the women may have a harder time breaking into leadership positions, but they may be tougher than the men so they actually make a pretty good job of it.

So now I can circle back to Fiona Harrison. I would like to think that Fiona's going to be running things and that, for example, chair of PMA would be her next step. I think she'd be a very credible division chair or even provost at Caltech, if she decides that's what she wants to do. I think she's not going to be happy doing that until she has scored the points she wants to score in her own scientific work. We have an associate professor here, Maria Spiropulu, and I believe she will probably do extremely well at high-energy physics. She's at CERN most of the time. I think she has the mark of a university president on her forehead. I think suddenly people are going to realize that not only is she a decent, very good scientist but she also has the right personality to run a university. I mean, she's already being propositioned by people in industry. [Google founders] Larry Page and Sergey Brin like her very much. Jim Simons, who ran Renaissance Technologies—the case of a mathematician who starts a high-frequency trading company and becomes a billionaire—he would just love to hire her.

ASPATURIAN: Well, might they succeed in wooing her away?

TOMBRELLO: No. I think what will win her away is an offer from some school of the University of Chicago type, where a president really matters. I think that would be something she would have to think about. Right now she's having too much fun looking for the Higgs particle and SUSY [super symmetry] particles and the rest of that. But I think in five years she'll be running something. That's been a big change: Watching women not only getting into science and becoming extremely successful at it but moving, in some cases, from doing science to running things. Of course, when you look at the statistics, you see that women now are the majority of college students and probably close to the majority of graduate students. It won't be long. Going back to [George Bernard] Shaw—Superman has now appeared. [Laughter]

ASPATURIAN: That's an interesting way to look at it.

TOMBRELLO: Well, Shaw had it right: *Man and Superman*.

ASPATURIAN: One of my favorite playwrights. You like Shaw?

TOMBRELLO: I do. My little running joke with Talulah Riley Musk came from when I told her I was Henry Higgins, and she said she was Eliza Doolittle, and that comes out of Shaw's play *Pygmalion*.

ASPATURIAN: Not to mention that musical *My Fair Lady*.

TOMBRELLO: Oh, well. That's the part we hum together. At some point, I was asking Talulah to do something, and she said, "I'm not going to bring you your shoes."

ASPATURIAN: "Eliza, bring me my slippers." [Actual line is "Eliza, where the devil are my slippers?"—ed.]

TOMBRELLO: "Bring me my slippers." That's what she said. "I'm not going to bring you your slippers." Are we through?

ASPATURIAN: Not quite. You mentioned the Bohemian Grove several times.

TOMBRELLO: Oh! That's an interesting little vignette. Summer of 2006; we're in Kauai. Bill Davidow is one of our neighbors there—he's a Caltech trustee. We got to know one another because we met him at the event at the Mauna Kea hotel in 2000 where I laid my trap for Gordon Moore. So anyway, Bill and Sonja Davidow and I knew one another; and Bill kept saying there's this wonderful place, Bohemian Grove. Well, the only thing I knew about Bohemian Grove is something I had read from—oh, the woman who wrote *Slouching Towards Bethlehem*?

ASPATURIAN: Joan Didion?

TOMBRELLO: Joan Didion. I love her writing. I *love* her writing! In one of her books, she'd thrown in this description about the Bohemian Club. So, I knew about it from that.

ASPATURIAN: Is this the place where the guys used to dress up in drag?

TOMBRELLO: Well, I'll tell you a little bit about Bohemian.

ASPATURIAN: I just want to see if we're on the same page here.

TOMBRELLO: It is a men's club. It has a city club in San Francisco. It has a redwood grove up near Santa Rosa, near Monte Rio, California. Inside the Grove, there are a bunch of little camps—you know, typically maybe twenty people in each. The membership total may be a few thousand. Politically incorrect; women aren't allowed. It tends to be mostly—you would say—conservative. The reputation it has is that its members are a bunch of the powers behind the scenes who run the nation—you know, Ronald Reagan, Henry Kissinger, George H. W. Bush. So anyway, Davidow gets this idea that I should go to the Grove, summer of 2006, as his guest. Stephanie says, "You don't seem to want to go." I said, "Boy Scout camp. I never liked camp." She says, "You're going." I said, "Yeah. Bill's a great friend. For Bill, I'll just do it and grit my teeth." I said, "I'm not going to take swimming lessons." [Laughter]

The other person who is pushing on me is [Victor] Tory Atkins; he was another Caltech trustee [d.2007]. I also met him for the first time at the 2000 Mauna Kea thing. He was a very young submarine commander out in the South Pacific and had a Silver Star for this—I didn't know that until recently. Tory didn't talk about any of that, didn't mention that he'd been in the navy, done the submarine thing. He ran some companies after the war, some of them connected with the defense industries.

So anyway, off to the Grove I go, in the summer of 2006. You go into this redwood grove, and they always talk about getting rid of "dull care." The outside world is the world of dull care, and Bohemia, the Grove particularly, is where you lose dull care. Well, the only way I could describe my opinion of that is it had to be bullshit. So we go in through the gate. I would say within a minute-and-a-half, two minutes tops, you're in these groves, these big redwood trees, with these little dirt roads running through these little camps. They're not dilapidated—well, a few are dilapidated—but they're rustic. And they line the roads.

Bohemia started with a bunch of performers. They got rich people to pay for it. So there are a lot of people there who perform. There are musicals. There are plays. There are impromptu productions of various sorts or just performances. There are even strolling minstrels who wander through the Grove and wander into a camp and, for a drink, will play for you. Walter Alvarez, Luis Alvarez's son, who's a very good scientist in his own right, is one of the strolling troubadours who appear in camp. Kevin Padian, who's one of the really fine paleontologists at Berkeley, usually joins Walter. They wander through, banjo and guitar in hand, playing for people. So suddenly, I discover that Bohemian Grove is idyllic. It's not Boy Scout camp at all—or it's sort of Boy Scout camp with hot showers and a good wine list. I realize this is an interesting place—I might actually have some fun here. Bill and I run into somebody at one of the camps. He's in charge of some of the scientific talks they give there, the museum talks. I give him some suggestions. He says to Bill—I've been in the Grove now maybe thirty minutes—"So when are you going to put Tom up for membership?" Bill looks at me, and I say, "Hey, go ahead."

Tory and Bill were my sponsors. There are three classes of membership. There are the real members. It takes *forever* to get to be a real member. But they have affirmative action. Affirmative action means sort of special conditions for academics. There's an even better deal for anyone who is a performer. Clint Eastwood belongs. Jimmy Buffett belongs. Some really good people belong, and they perform, and they love to perform. So then you fill out this application form, send it in. Presumably you try to get people you know to write letters on your behalf, and your sponsors spend a lot of time and money trying to put together this package. But then at the end of that you have interviews. It's like getting into a fraternity—not that I've ever been in a fraternity. So I drove up to the Bay Area with my wife. We're staying with my late daughter and her husband and their twins. I do something like eighteen interviews in a day. Of course, I show I have a sense of humor, which I didn't really have. They would say, "Is there anything about Bohemia you don't like?" And I said, "Yeah, there's one thing. It's this affirmative-action policy. As soon as I get in, I want to stop all this letting academics in. I don't want to go to the Grove to be around a bunch of academics." [Laughter] They thought, what a great sense of humor. And I thought, Ah, I mean it. Although the academics there are interesting—Stan [Stanley] Prusiner's there; of course, David Baltimore. At the point I got in, the only Caltech faculty person there was Baltimore, followed then by Elachi. Then, I think, followed by me—Andy Lange was trying to get in. He killed himself before he did his interviews, but he had filled out the application for it. They liked him, and I think he would have liked it there. It would have been good for him. We academics don't get to vote, because we are affirmative-action candidates and we pay a little less for some things.

Once you're in, you're just a member, and now you start, in the phrase, "sleeping around." Sleeping around means people invite you to various camps or you figure out a way to get invited to various camps. You spend a weekend at the camp and there must be a lot of discussion behind the scenes of where people belong. I ended up in a camp called Sons of Toil, which probably has the most academics of any camp at the Grove. They're interesting people. There are some entertainers in it, too. The politics run from liberal to so far right it's around the curve of the universe. The most conservative human being I know is Walter Williams, an African American from Washington, who occasionally sits in for some of the real conservatives on, you know, TV talk shows. He is clearly in a whole class by himself: There is nothing that can't be cured by less government. Mike Garrett, who is a friend of mine and who used to be athletic director at USC [See also Session 3], is there. He's quiet about politics, but I know perfectly well he is not with his fellow African American, Walter Williams. A previous camp captain, when he met my wife, they realized they had both done something big. He had been a brain surgeon who realized that a lot of head injuries occurred because people didn't have seat belts in cars, and he found himself a congresswoman and got the seat belt law passed.

ASPATURIAN: And Stephanie, of course—

TOMBRELLO: —had done the same thing for car seats. It's interesting. These people have all *done* something. When they asked me to be a member of Sons of Toil, I said, "You know, you people are making a dreadful mistake. All of you are interesting people who have done important things. All I do is teach little kids and enjoy it, or teenagers and enjoy it." I think they let their standards down a bit. But I enjoy it. I enjoy being there. It's a different environment. I remember taking Mike Garrett to another camp called Isle of Aves—the Island of the Birds. I introduced him to the bartender; he didn't quite get the name. The guy's name is Jeff Warren. And Jeff says, "Oh, you're Mike Garrett. Wow!" He says, "You know, I have a story. I'm a Berkeley grad. Some years ago, when you were playing, there was going to be a game between USC and Cal, up in Berkeley. I found these two ladies wandering around the airport in Oakland and asked if I could help them. And they said they needed to get to the game because one of them was your mother and one of them was your sister, and," he says, "I made sure they got to the game. And then you proceeded to trounce us." Mike expressed his gratitude, and as we left the camp, he said, "Who was that guy?" I said, "His name is Jeff Warren. His grandfather was Earl Warren." He said, "Oh, my God. One of my heroes." Of course, one of the stars of that camp is a guy named Jesse Choper, and Jesse and I are really good friends now. He used to be dean of Boalt Hall, the law school at Berkeley, and he of course had clerked with Earl Warren. Every year, there's a little quiet invitation to lunch at Aves, where Jesse talks about what's happened in the Supreme Court for that year. At another camp, Hillside, also quietly advertised, our very own trustee, Bobby Inman, gets up and summarizes the state of the world—just standing there without any notes. It takes about an hour and a half and is absolutely spectacular. The place is so full for his talk that I'm thinking the engineering had better be good, because otherwise the damn deck will probably collapse because there are so many people on it. There's standing room only to hear Bobby.

That's the kind of stuff that goes on there. You hear things that you wonder about. I'm a great fan of the writing of Jeffrey Toobin, the *New Yorke*r reporter. He came and gave a good talk, but he has feet of clay. He doesn't listen. At the Grove, you can't have cell phones. You can't have recording devices. You can't have cameras unless you're one of the Grove photographers. Some of them have Pulitzer Prizes, so they sort of come trained to do really good photography. They let those people run loose and take pictures. But the directives are clear—especially about cell phones When you go through the gate, you see a bunch of cell phones up on a beam, with stakes driven through them. So you see a graphic example of what happens to cell phones. So Jeffrey's in there, and his cell phone rings. They say, "Jeffrey, turn it off and don't answer it. Turn it off." Then a day later it rings again, and the Grove is full of people who don't defer to authority or care much about who is important and who isn't. He was escorted to the gate and kicked out. [Laughter] You're supposed to go there as a real retreat. If you want to communicate, you go off-site, fire up your cell phone, and maybe you can communicate, but at the Grove that part of life is back to a more primitive state.

ASPATURIAN: Are women ever invited as guests?

TOMBRELLO: Only for the picnics. They have two picnics a year, one in the spring, one in the fall, and I have taken people—women people—to the picnics. Frankly, I think the atmosphere improves with women there; the women add a lot. I arranged to take Talulah and Elon [Musk], because when I first met Talulah she was *very* interested in the Grove. She says, "Never any women." I said, "Well, there are waitresses." And she says, "I can play a waitress." I said, "Talulah, I do not want to look up over my morning blueberries and see you. Would you like to go to a picnic?" So, we took in a picnic. The first thing she wanted to do was see one of the rooms. We get it opened up. She looks at it and says, "Just like girls' boarding school in England." I think that particular visit cured her curiosity about the Grove. She had a great time that day, and now it holds no mystery as far as she's concerned. We took a friend of ours up there at another picnic. She didn't expect to know anybody, but she almost immediately ran into a friend of hers who is sort of engaged to a Grove member artist we know. You see a lot of different people there. You meet people that you have known in other lives. I've run into people from Schlumberger. I reconnected with Ed Knapp, my friend from Los Alamos and the National Science Foundation. We had some discussion of Ed. He died of pancreatic cancer about a year ago [August 2009].

ASPATURIAN: How large is the membership?

TOMBRELLO: A few thousand. Of that, probably 125 academics, and probably something of the same sort, or maybe a little bit higher, of the performers and artists. Some of the performers are actually artists who, when they're there, just start painting backdrops. There are some very interesting people who just do that, and, of course, some of the photographers. Interesting place. It wasn't what I expected, and I enjoyed it a great deal. Women had their own version of it on the East Coast. Sonia Sotomayor belonged, but then, I guess because of political pressure, resigned from it. I'd like to see some transition—that sometime in the distant future, Bohemian Grove becomes coed.

ASPATURIAN: Like Caltech.

TOMBRELLO: Right!

ASPATURIAN: A model for it to emulate.

TOMBRELLO: Well, that was interesting, because somebody had made a suggestion about somebody to give a museum talk, and I said, "Well, I have to point out to you that he is subordinate to a woman who basically led the research and is really the person who speaks for it. I think we would look kind of foolish to invite him rather than her, and you can't invite her." I said, "I think you have to forget the topic unless you can figure out a way, sometime in the future, for us to start inviting women to give talks."

ASPATURIAN: Maybe it's time. It's the 21st century.

TOMBRELLO: It's just going to be increasingly embarrassing, because there are more and more cases where women are going to be the ones who are leading particular things that are going on. But some of the talks are very, very good. Walter Alvarez gave an extraordinary talk on, sort of, deep time—looking at history with some deep vision of it. Then there are some like Bill Gates, who gave a talk that everyone agrees was the second worst talk in the history of the Grove.

ASPATURIAN: How come? Is he just not a natural speaker?

TOMBRELLO: No, it wasn't that. He wasn't so much self-important as he was sort of oblivious to the fact that this was much more of a personal talk about Bill Gates. It just didn't resonate with anybody. Self-importance doesn't resonate with anybody. Charlie Munger, you know, [Warren] Buffett's business partner—

ASPATURIAN: Whom you interviewed a few years back here in Beckman Auditorium.

TOMBRELLO: Yes. But you see, he wasn't being interviewed; he was giving a talk. That's one thing. When I set up my arrangement with how I was going to deal with Charlie Munger, I realized he needed an editor. That he was a brilliant man, and if you could keep him on script, people would love it. But the trouble was, he wrote and delivered his own script at the Grove, and apparently—I didn't hear it—it was bloody awful. They just resolved that Charlie Munger was hopeless. Well, I studied Charlie Munger carefully before I did that interview and realized that if you keep him on message, he's great. If you let him drift, no one's going to be happy.

ASPATURIAN: It sounds like you've heard a lot of talks up there. What would you say were the three best, if you had to single them out?

TOMBRELLO: Walter Alvarez, probably. That was clearly a very good talk. The first time I was there, we heard a talk by a teacher who was, I think, teaching in L.A. There were grown men crying at the end of that talk. He had gotten a bunch of kids at what must have been roughly junior-high level and changed their lives. He read a letter that one had written for her admission essay to college in which she said, basically, "My life began in the seventh grade." It was a great talk about how you influence children. Peter Peterson gave a great talk.

ASPATURIAN: Who is he?

TOMBRELLO: I think he was in finance or banking, but he started something called the Peter G. Peterson Foundation. He put a billion dollars into it. He wants to change the nature of political effectiveness in this country. And anybody, of course, who is in California, knows we desperately need somehow to get beyond where we're stuck. If you look at what's been happening in the U.S. Congress for the last couple of years, you realize that Peterson is clearly talking about exactly the right thing. We have somehow got to get beyond the politics and get on with what society needs. It's very interesting that he came into it from the business side and is putting up a substantial amount of money to try to make it work. That was impressive. Rupert Murdoch came and gave a talk that was barely OK. Nobody was blown away by it, but nobody said it was the worst talk in the history of the Grove, either. You get to hear interesting people. [Arnold] Schwarzenegger came and gave a talk. It was a solid B-minus, C-plus talk. You didn't feel you should have stayed back in camp and had another glass of wine, but it wasn't one you were going to take home and tell the wife and kiddies about.

ASPATURIAN: So there's a spectrum.

TOMBRELLO: There's a spectrum, and some of the impromptu talks that members are asked to give are extremely good, too. They wanted Mike Garrett to get up and talk about the economics of college sports. Well, Mike had been running an $80-million program, and we didn't know how the economics of that worked. He got up and in about an hour gave us a very clear picture of what it's like to raise $80 million a year from scratch, and what the ingredients in that are; and how do you play it. TV, of course; selling TV rights is a huge thing. So if you don't have a winning team, then you've got problems, because this is a major income stream, and so part of his challenge was keeping USC up there where you make enough money to cover *all* the other sports that don't make money. Football makes most of the money and basketball certainly helps; beyond that they're all losing money, effectively. You've got to support those other sports, and you've got to support women's sports. Now, if you have a winning team, like University of Connecticut [women's basketball team], which just ended their long winning streak last night against Stanford—they've won, I think, over seventy straight games—even women's sports will make money, I'm sure. People love to come out and cheer for winners. That's why we have the kinds of trustees we have. I think our trustees think they're backing a winner. My late father-in-law, Robert Merton, created an idea called the Matthew effect, from the Gospel according to Matthew. Well, you know, basically, the rich get richer, and the poor get nothing, and basically, nothing succeeds like success. If you're successful, you'll become even more successful. If you're not successful, well, don't count on reversing it.

ASPATURIAN: Unless, of course, your name is Bernie Madoff.

TOMBRELLO: That's a very interesting story, and I hope someday they can tell how it actually worked, because he fooled a lot of people. I mentioned Jim Simons. When he was a mathematician, he did some of the mathematics for the Maxwell-Chern-Simons theory, which underlies string theory. Then he formed the Renaissance Technologies hedge fund and became a billionaire. He was a trustee for the State University of New York at Stony Brook, and he got them into an investment in Bernie Madoff's fund. I think he felt sufficiently chagrined about his advice that he ended up giving them a lot of money. So there's probably a net win for SUNY, but— Madoff fooled a lot of people. You know people will sometimes believe in perpetual motion, even though it's impossible.

ASPATURIAN: Well, he fooled a lot of people, but there were some very clear warning signals, and they were repeatedly ignored. It's almost like the *Challenger* disaster again.

TOMBRELLO: It's like a lot of things—you have Brooksley Born warning the Clinton administration in 1997, when she was head of the Commodity Futures Training Commission, that the derivative thing needed control. And by the way, she had written a set of proposed controls; she got beat down by Larry Summers, Bob Rubin, and Alan Greenspan. And in 1998, my wife's baby brother's little startup, Long-Term Capital Management, went belly-up. They had a trillion dollars in play, and it was based on about $5 billion in capital. Fortunately, only the $5 billion was lost and not the $1.25 trillion. But Brooksley said that was a close call and we need more regulation. They beat her down again and she ended up resigning from the commission. Last year, she won the Profiles in Courage Award. I would love to see that woman on the Board of Trustees. I think she's extraordinary. Where is she now? She got her law degree at Stanford. The whole time—she's probably roughly my age—she was reminded that she was taking up a space a man could be in. Of course, she was the top of the class and edited the law review.

ASPATURIAN: What do you think about the trend that has a lot of gifted graduates in math and in physics heading for Wall Street, where they are putting together these immensely complicated financial algorithms?

TOMBRELLO: I think some of them, not all, have gotten in there because they came out of high-energy physics, where the number of faculty positions is relatively limited. They feel that they have an alternative if they can go somewhere like Wall Street and use some of their mathematical skills. I don't think that's *all* of it. The other part is the challenge of "If so-and-so can do it, I can do it." I have a former student, undergrad, named Sebastian Maurer, who went from Stanford to D. E. Shaw on Wall Street. He has been there almost ten years now. He's awfully bright. I don't think he looks on this as something he's going to do forever. It's something you do until you think of something else you might want to do. Another kid out of Physics 11 is, I guess, a vice president at Morgan Bank and also does derivatives and structured instruments, like CDOs.

Vineer Bhansali, who was my TA and research student back in the 1980s, is VP for PIMCO, I guess in Newport Beach. He runs their research department and has published a couple of books with Mark Wise on finance. Not only are they successful at this stuff, sometimes they drag the old professor back in, as in the case of Mark Wise. Rich [Richmond A.] Wolf, who got his PhD in geochemistry here [1997], is a VP at Capital Group.

ASPATURIAN: He went there from Tech Transfer [Office of Technology Transfer] here.

TOMBRELLO: From Tech Transfer. He was very successful at Tech Transfer. He was one of the people who Larry Gilbert's vision identified and trained. Larry is one of the few people I know who came in and totally changed the culture at Caltech. It was fun working with him for the fourteen years I was technology assessment officer. I will take no credit for the vision of Tech Transfer; that was Larry. I was there, picked by Larry, to keep things from running afoul of campus traditions and standards that Larry did not understand. He'd worked for John Silber at Boston University, and that, of course, gives one a certain freewheeling style. We had some very interesting experiences. We may as well get some of them down if you have a few more minutes.

ASPATURIAN: I do if you do.

TOMBRELLO: One of the best—I'm going to give you the ones that make me look good, obviously, that's the way things go—is about a company run by a Caltech professor, and he didn't have a clue.

ASPATURIAN: The name?

TOMBRELLO: No. Yeah, it was Harvey Newman [professor of physics], come on. It was a beautiful little idea in teleconferencing. Very efficient teleconferencing. Developed with the taxpayers' money for CERN. But Harvey is not a natural businessman. In fact, you might say he is an unnatural businessman. He started a small company based on this idea, and basically it created such a mess that there was a chance of a lawsuit with some of the other investors. Larry and Rich asked me what I would do. I said, "I know what you're going to do. We're going to give our stock away—all of it. We're going to give it to some of the other people who've invested in the company—all of it, every bit of it." They said, "Why?" I said, "If we don't have any stock, we can't be sued." So it worked like a charm.

Here's another very interesting case. There was a patent on an asynchronous microprocessor from campus. It was being willfully violated by Intel Corporation. And we were told quite specifically by somebody at Intel that we wouldn't dare touch them because it might bother Gordon Moore. We sort of accepted that argument. Rich said, "What are we going to do?" I said, "I know what I'm going to suggest. I want you to find a predatory firm who will buy that patent." He says, "I assume you mean license the patent." I said, "No. I don't want to license. I want all my fingerprints off that patent. I want to sell that patent." Rich did his homework and he found Nathan Myhrvold, who runs this company called Intellectual Ventures. Nathan was first chief scientist at Microsoft, and one day at lunch, up in Bellevue, Washington, Rich and I sold the patent for $600,000. Somebody said subsequently, "Well, what did he do about Intel?" I said, "I don't want to know." Whatever he did, that was Nathan's thing. There was no way we could make money out of it, because we weren't going to hold Intel's feet to the fire. If Nathan did it, hey, that was Nathan. I don't want to know about it. [Laughter] So there were some very interesting things where having a Sicilian heritage was not exactly—

ASPATURIAN: A liability.

TOMBRELLO: There were a lot of things that were fun. There were lots of things that were frustrating. But for the most part I think we worked very well together to try to keep this within the bounds of reality. To see that professors were not going to misuse their grad students. That they were not going to mix money. We really had very few problems, almost no problems, with it. I enjoyed it very, very much. I enjoyed working with Larry, who's clearly a genius, because before Larry came there had been so little entrepreneurial activity. But see, I was ready for it, because I had been so surprised when I came back from Schlumberger. A friend of mine named Neal Lane, who had taken over the National Science Foundation or was about to, came out to campus, and we were having breakfast with a bunch of students. He asked them what they were going to do. You know, I expected the standard answers: "I'm going to get a postdoc." "I'm going to go to a national research lab" "...going to an industrial research lab." That sort of covers all the students I'd ever known at Caltech. And then I heard a bunch of these kids say, "I'm going to go out and start a business." I thought to myself, "Where did that come from? These two years I've been away, the whole world has changed." I know why it had changed. We had a trustee whose name is Gordon Moore, and he was running around giving talks at Caltech saying it's all right to be an entrepreneur. Now, did the faculty listen to it? Not at first. But the students got the message *immediately*. The Pied Piper was leading those children away.

ASPATURIAN: What an interesting story.

TOMBRELLO: So when Larry came—

ASPATURIAN: Did David Goodstein bring in Larry?

TOMBRELLO: Goodstein had a lot to do with it—he and Everhart had the idea that this was important, and I think Goodstein really ran the search, or found Larry. It was a brilliant choice. Larry was good, not only in— Brilliant in picking me to help him. [Laughter] What can I say? But no, he was good at talking to the faculty, figuring out which faculty members would fit into this and how not to offend the ones that didn't want to fit into it. Do you remember that Jack Roberts was very much against the whole thing? Jack and I have been friends for a long time. Jack said to me, "Tom, you're wrong about this." And I said, "Let me explain, Jack. We've got a product at Caltech. We've got a fantastic product—it's called our students, and we sell it. And we sell them high." And I said, "You know, the markets where we used to sell the students are changing. There are not as many places you can sell a professor. Research labs for industry are disappearing. The national labs have no vision for the future." I said, "There's one new market—the startup game; the small companies." I said, "We've got to have a market for these kids. And this is the market, and it's going to pay off. I don't know if it will pay off in the companies we start at Caltech. But it's going to pay off in the long run when there are going to be more people like Gordon Moore, and they're going to end up giving us a lot of money because they got their ideas here." Well, it had happened with [Arnold] Beckman, of course, who developed the pH meter here. Gordon didn't develop anything here, but Gordon's been extremely grateful to Caltech. Jack said, "Well, I'm not sure I agree with you, but, you know, I understand the motivation that we're doing it for the students."

And it happens two ways with the students. They see it going on around them and they know it's OK. And of course, they develop the kind of context they need to actually try to do it. Caltech tries to help them with it. I had a student—Michael Woods, who graduated a few years ago—who had an idea for a company. I'm a Gnome, which I consider one of the great honors I have—belonging to the Gnomes at Caltech. I said, "Hey, come to the Gnome Christmas party; I want to show you how to network." Within probably an hour of just walking around with a drink in his hand, talking to people, he got all kinds of ideas of how you begin to start a company. Free, hey! This is seriously good advice. They got very interested in him, because they're very devoted to Caltech. So the culture changed, and it changed in a way that I'd like to think will never go back, because it opens up a whole new range of possibilities for our students. And, in addition, Caltech has made a hell of a lot of money off Tech Transfer. There have been no scandals. I think I had a lot to do with no scandals.

ASPATURIAN: You've kept things on the straight and narrow.

TOMBRELLO: Larry always listened. Larry never disagreed about that. We wanted to avoid having to explain why something bad had happened. They've now sort of turned it over to [vice provost] Mory [Morteza] Gharib, in the provost's office, and I hope he does it well. It's not a job that you spend every waking moment thinking about, but there's so much to lose that you do have to pay attention to it. You just have to have a sensitive nose for, oh, maybe you should look at *that*, just to see exactly what's happening there. But one of the big changes in my lifetime at Caltech has been the influence of Tech Transfer on the whole culture. Because before Larry came, this place was a different place. Professors did start businesses: [Charles] Richter and [Frederick] Lindvall [professor of engineering, d. 1989] had a business. [Amnon] Yariv had businesses. A number of people had businesses, but they were not really connected to Caltech. It was because a professor had a vision and did it anyway. And we had a bunch of crazy rules.

ASPATURIAN: Yes, many.

TOMBRELLO: Some of which were created by Jack Roberts when he was provost, which we just had to get rid of. They didn't make any sense. They didn't make any sense for the world we were moving into, and the world we're likely to stay in for a long time. It's funny when we start looking at some of the younger trustees we've got. A lot of them come out of that world; a few of them are ours. But a lot of them are attracted here because they see Caltech doing pretty well in this area of taking science and turning it into something useful. The latest example is a little company started by David Baltimore, Axel Scherer, and this grad student named George Maltezos, which just got sold six months ago for $110 million. Not so shabby. Caltech made money out of it. I'm sure the inventors made money out of it. And it sent a clear signal.

ASPATURIAN: Axel Scherer. Is that the field-testing malaria chip?

TOMBRELLO: Yes. Well, it does everything. You find the substrate, and it will test for many things. David and Alex have each gotten $2.5-million grants from the Gates Foundation. I think they're going to develop a test for tuberculosis, because roughly the same technique can be modified to do that. These are big-ticket items.

ASPATURIAN: They sure are.

TOMBRELLO: You can see when the Gates Foundation comes in, they see it as having a big societal good attached to it. I think it will be a case of doing well by doing good.

ASPATURIAN: When a sale like that is made, what percentage of the revenue comes back to Caltech?

TOMBRELLO: Depends on what the original setup was. Previously Caltech did rather poorly. We took license fees. That worked well with [Leroy] Hood's DNA sequencer; but Caltech never took equity. With small companies, there's an interesting trade-off on whether it's better for them to give you some percentage of the equity in the company or pay you license fees. That's where Larry really was important, in trying to work out with the company what the proper mix was. Typically, at the maximum, Caltech will have 5 percent of the stock, which can be a lot of money, a lot of money—but not to be greedy. You know, the key was that Caltech essentially had put nothing into these things; 5 percent, when you put in nothing, is not shabby. It's *good!* There may be license fees as well, and Caltech continues to own the patents except for that one case where we sold it. [Laughter] I'd love to think that Nathan made some money out of that. Someday I will ask Nathan, because I do see him periodically. In fact, I'll probably see him in about a week; Maria Spiropulu has organized this "Physics of the Universe Conference" again this year.

ASPATURIAN: Here on campus or off-site?

TOMBRELLO: It will be one day at SpaceX and one day here.

ASPATURIAN: SpaceX being?

TOMBRELLO: Space Exploration Technologies; it's owned or run, founded by Elon Musk. In the last year it has successfully launched two million-pound thrust rockets—the Falcon 9s. NASA is now becoming a big contractor with it, and it fits President Obama's paradigm, which was maybe we can go to private companies to supply services in the big rocket business. The president loves Elon Musk, and he should. Elon is a very interesting model for the future of industrial growth in the country, between SpaceX and Tesla Motors, and of course SolarCity—the company that installs solar cell systems on houses and businesses. I think it's a business model that people should not ignore.

ASPATURIAN: Is he originally from Russia?

TOMBRELLO: He is originally from South Africa. The story can be found on the Web, so I won't spend a lot of time on it. He ran away from home as a teenager. His mother had Canadian citizenship, so he got to Canada. Got himself into undergraduate school at the University of Pennsylvania and graduated with degrees in physics and, I believe, business. And the rest is history. First, there was some little Web publishing thing, which probably brought in a couple of hundred million dollars, and then there was PayPal. Now, of course, he's into manufacturing. I think the country, considering the loss of manufacturing jobs, ought to be paying very close attention to what he's doing. Of course, you know, I'm a fan of Elon. But, as I've said in many interviews, I'm a critical fan. But being nine years old, I'm enthusiastic about a lot of things.

ASPATURIAN: That's right. It helps to be perpetually nine years old.

TOMBRELLO: Underneath it all, I'm not willing to accept stuff that I don't think meets standards. So far, everything Musk has done certainly meets any standard I could set.

ASPATURIAN: Well, on that note—

TOMBRELLO: Good! This has been delightful.